MONTHLY NOTICES

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EDWIN DUNKIN, F.R.S., President, in the Chair.

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Thomas Cunningham Porter, B.A., The Grammar School, Carlisle;

Captain B. Thomson, R.N.R., Ghyllbank, St. Helen's, Lancashire;

H. H. Turner, Esq., B.A., Royal Observatory, Greenwich, S.E., and Shurland House, Humber Road, Westcombe Park, S.E.; and

James Wigglesworth, New Parks House, Falsgrave, Scarborough;

were balloted for and duly elected Fellows of the Society.

On the Proposed Change of the Astronomical Day. By Professor Simon Newcomb.

The recommendation of the recent Meridian Conference at Washington, that the astronomical and nautical days be arranged as soon as practicable to begin at mean midnight, has received so much attention at the hands of English and American astronomers, as to indicate its possible adoption in some quarters without due consideration of the radical character of the proposed change. I therefore desire to set forth my reasons for counselling caution in this proceeding.

Outside of statements of purely astronomical data the proposed universal day may well prove a great convenience. In the reckoning of physical and meteorological phenomena there has hitherto been no recognised standard of time in general use. For such purposes I see nothing better than the general adoption of the day beginning at Greenwich mean midnight as proposed by the Conference. Moreover, could the human race commence its work anew by obliterating the past, there would be a certain advantage in having the astronomical reckoning of time to correspond with the universal time in other departments of science; and this advantage would probably more than compensate for the loss of that simplicity and generality which inheres in the system of counting the hours from the moment when the hour-angle of the mean sun is zero. It is not plain, however, that the lack of accordance between the two reckonings of time will lead to any serious trouble or confusion. Astronomical ephemerides and observations form, so to speak, a department by themselves, which none have occasion to enter except those who are thoroughly conversant with the two methods of reckoning time. I can recall no instance of trouble or confusion in the past arising from the difference between the astronomical and the civil day.

So far as astronomical observations and ephemerides are concerned, the change, if made at all, must be made throughout, as it would be intolerable to have two methods of reckoning time in the same connected set of publications. It is, then, subject to the objection that it would cause great trouble and confusion, not only to ourselves, but to future astronomers, through as many generations as made use of our observations and ephemerides. A glance at the *Nautical Almanac* will, I conceive, convince us of this. If we are to have but one system, then, on page 11 of the month the data must be given for Greenwich midnight instead of noon, and the sidereal time must be that for midnight. If we do not do this there will be additions and subtractions of 12 hours to be performed by all future generations in changing sidereal to mean time. On page 4 the columns for midnight and noon must be interchanged. On pages 5 to 12 the hours must be reckoned from midnight. The consequence will be that

whenever the astronomer of 20 or 100 years hence has occasion to refer to the ephemeris, he must know and bear in mind which reckoning of time is adopted, else his place of the Moon will be taken out 12 hours in error. The same remark will apply to all data for which hours of time are given. To continually remember anything of this sort is a mental burden which no one can always be sure of carrying. If we could be sure of this, accidents and inadvertences would be almost unknown in the world. The ephemeris of the planets would, for consistency, have to be given for midnight instead of noon. Thus, there would be a break of half a day in the series which now progresses regularly at 24-hour intervals.

It might, indeed, be said that this ephemeris could just as well be continued for noon as given for midnight. But this would be simply a half-way step, and would lead to the difficulty that in interpolating to any required number of hours universal time, the 12 hours would have to be subtracted whenever a place of the planet had to be interpolated. I pass over a number of other points connected with the change; among them the fact that the instructions and precepts respecting time given in our books on practical astronomy will have to be changed. It is a general rule that the judgment of men, on the whole, errs as much on one side of any question as on the other. But my experience leads me to think that a decided exception to this rule arises when the question is that of changing a well-established and consistent set of methods and habits in organised work, and that we always under-estimate the trouble and confusion such changes will cause us. It is very clear to me that the change is one which ought not to be made at all, unless some stronger reason for it than is now presented shall be pointed out. In this case it ought to be made at some common fundamental epoch by an arrangement among the astronomers of the world. The beginning of the coming century would be a very good epoch, and would allow about the right time for consideration. If a ten years' continuance of the present system shows that it needs amendment, the change can then be made with less trouble than at any other time.

Faint Stars for Standards of Stellar Magnitude.

(*Extract from a Letter from Prof. Pickering.*)

The charts accompanying this circular represent four out of twenty-four regions from which it is proposed to select certain stars for standards of the magnitude of faint stars. Additional information with regard to the plan is given in the printed Reports sent herewith. The regions represented in these four charts are those from 2^m to 6^m following the bright stars γ *Pegasi*, ε *Orionis*, η *Virginis*, and η *Serpentis*. Each region extends 5′ north and 5′ south of the Declination of the corresponding bright star.

It is desirable that these charts should be made as complete as possible, and it is hoped that astronomers having the use of powerful telescopes will assist in accomplishing this object. They will confer a favour upon the Observatory of Harvard College by comparing these charts with the regions which they represent, and marking upon them the places of any additional stars which may be visible. Some indications of the comparative brightness of these stars would also be desirable.

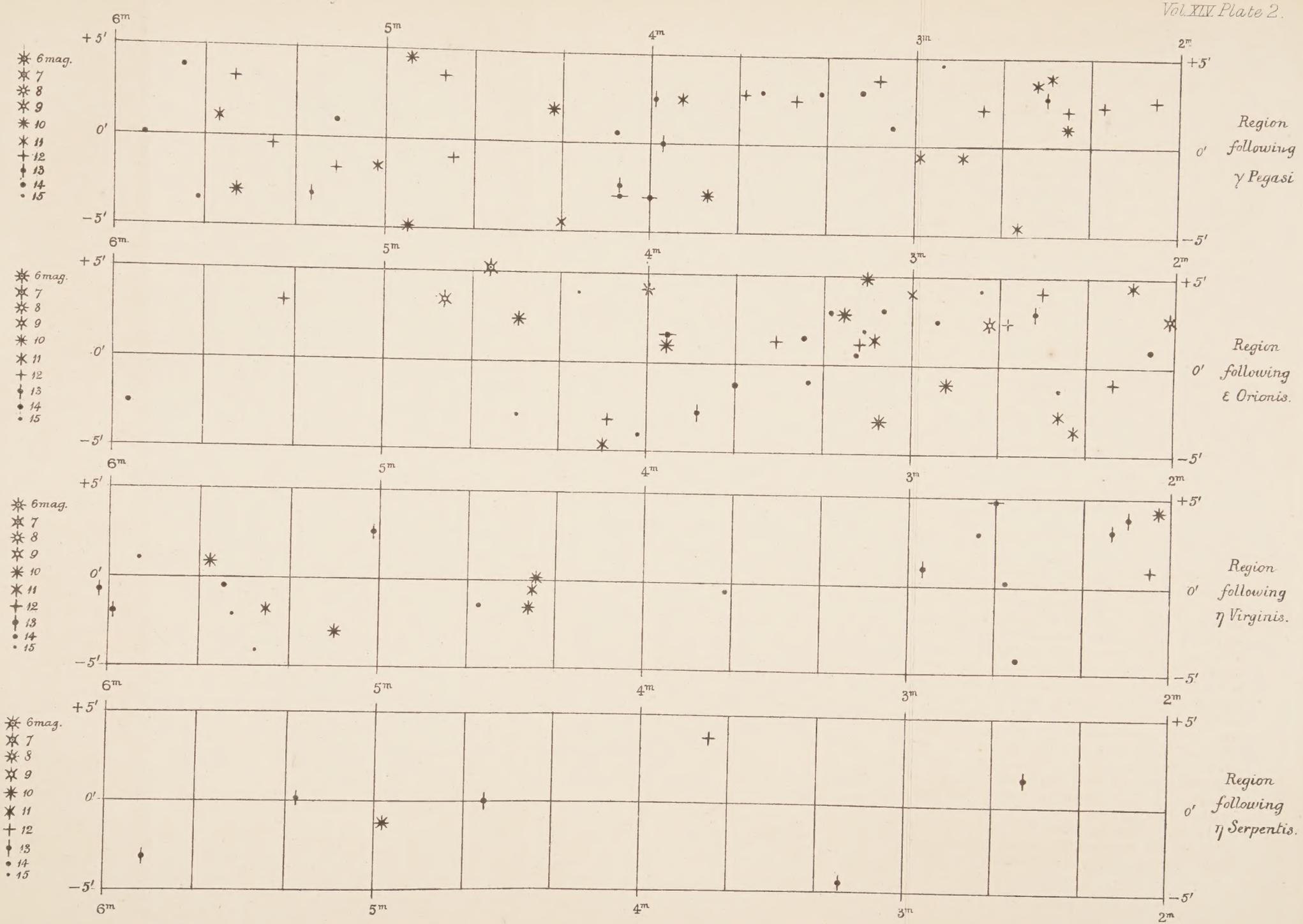
Astronomers who may be disposed to take part in this work are requested to send the chart of any particular region, as soon as possible after its revision, to the address given below.

Harvard College Observatory,
Cambridge, Mass. (U.S.):
1884, *Dec.* 17.

Second Report of the Committee on Standards of Stellar Magnitudes.

The first Report of this Committee (*Proc. Amer. Assoc.* XXX. p. 1) included a plan for the determination of standards for stars fainter than the tenth magnitude. Twenty-four bright equatorial stars were chosen, and the standards were to be selected from the regions following them from two to six minutes of time, and not differing in Declination from the leading stars by more than five minutes of arc. The observations described below have been made at the Harvard College Observatory unless otherwise stated. The light of each of the leading stars has been determined on from seven to eighteen nights with the meridian photometer. Charts have been constructed of all the stars visible with the 15-in. telescope, in all but three of the regions from which the standards are to be selected. Most of these charts have been submitted to a careful scrutiny with the 15-in. telescope of the Washburn Observatory. An important test of the completeness of the charts is thus afforded.

In the following table three successive columns give the names of the twenty-four leading stars and their approximate Right Ascensions and Declinations for 1880. The next two



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columns give the number of nights on which they were observed with the meridian photometer, and the resulting magnitude. The details of these measures and a comparison with various other determinations of their light will be found in the *Harv. Observ. Annals*, vol. xiv. The last columns give the number of stars in each of the charts, and the corresponding number of stars contained in the same portions of the *Durchmusterung*.

Stars suitable for standards must next be selected by the help of the charts. The light of these stars should then be measured in as many different ways as possible. The Committee will be much indebted for aid that may be rendered them in this portion of their work. The early publication of the charts now becomes a matter of importance, as it would permit their immediate use for various purposes.

Name.	R.A. 1880. h m	Dec. 1880. ° ′	No. Nights.	Phot. Mag.	Stars on Chart.	D. M. Stars.
γ Pegasi	0 7·1	+14 31	13	3·04	49	3
θ^1 Ceti	1 18·0	− 8 48	12	3·77	27	
α Piscium	1 55·9	+ 2 11	12	3·99		1
α Ceti	2 56·0	+ 3 37	11	2·68		2
γ Eridani	3 52·4	−13 51	10	3·05	30	
α Tauri	4 29·0	+16 16	16	1·00	19	3
ϵ Orionis	5 30·1	− 1 17	16	1·76	42	6
γ Geminorum	6 30·8	+16 30	17	2·00	150	5
α Canis Min.	7 33·0	+ 5 32	15	0·46	96	3
ϵ Hydræ	8 40·4	+ 6 51	7	3·58	64	4
α Leonis	10 2·0	+12 33	15	1·42	39	1
θ Leonis	11 7·0	+16 5	10	3·47	24	3
η Virginis	12 13·8	+ 0 0	10	4·05	23	2
α Virginis	13 18·9	−10 32	13	1·23	30	
α Boötis	14 10·2	+19 48	13	0·03	25	1
β Libræ	15 10·5	− 8 56	14	2·74	39	
δ Ophiuchi	16 8·1	− 3 23	11	2·77	48	
η Ophiuchi	17 3·5	−15 34	10	2·62	100	
η Serpentis	18 15·1	− 2 56	10	3·35	7	
δ Aquilæ	19 19·4	+ 2 53	14	3·46		3
θ Aquilæ	20 5·1	− 1 11	10	3·39	110	2
β Aquarii	21 25·2	− 6 6	10	3·14	52	
α Aquarii	21 59·6	− 0 54	10	3·16	48	0
α Pegasi	22 58·8	+14 34	18	2·61	29	3

Note on the Track of the Total Phase in the Solar Eclipse of 1885, Sept. 8, in its passage across New Zealand. By J. R. Hind, LL.D., F.R.S.

The only land traversed by the central line in the total eclipse of the Sun in September next will be the southern part of the north island and the northern part of the south island of New Zealand, where the eclipse occurs early on the morning of the 9th, civil reckoning. The following are points in the central line corresponding to the annexed Greenwich mean times, with the duration of totality on that line. The positions of Sun and Moon, and the semi-diameters, have been taken from the *Nautical Almanac*:—

Greenwich M.T.		Long. E.	Lat. S.	Duration of totality.
	h m	° ′	° ′	m s
Sept. 8	8 2	171 28·5	40 28·5	1 50·3
	8 3	172 47·2	40 33·3	1 52·5
	8 4	173 59·1	40 38·8	1 54·6
	8 5	175 5·7	40 44·8	1 56·5
	8 6	176 8·2	40 51·3	1 58·4

On referring these points to the Admiralty general chart of New Zealand, it will appear that the central eclipse enters the south island between Wanganui Inlet and Cape Farewell, and leaves the north island a little north of Castle Point, near which will be the most favourable position for observation. Taking the position of Cape Farewell in longitude 172° 43 E., latitude 40° 30′·5 S., the total eclipse begins Sept. 8, at $19^h\ 32^m\ 49^s$ local mean time, and continues $1^m\ 52^s$, with the Sun at an altitude of about 15°. For any point near Cape Farewell the Greenwich mean time (t) of beginning and ending of the total phase will be given by the formulæ

$$\cos\omega = -113{\cdot}1590 - [2{\cdot}09859]\sin l + [1{\cdot}62923]\cos l\,.\cos(\lambda - 162°\ 3'{\cdot}3)$$

$$t = 8^h\ 58^m\ 31^s{\cdot}1 \mp [1{\cdot}74986]\sin\,.\,\omega - [3{\cdot}36498]\,.\sin l - [3{\cdot}85019]\,.\cos l\,.\cos(\lambda - 146°\ 20'{\cdot}4)$$

Here l is the *geocentric* latitude, and λ the east longitude, from Greenwich, taken positive. The upper sign is to be used for the beginning, and the lower sign for the ending. The quantities within the square brackets are logarithms.

The position of Castle Point, read off from the chart, is in longitude 176° 14′ E., latitude 40° 54′ S., and a direct calculation gives the commencement of totality at $19^h\ 50^m\ 8^s$ local mean time, and duration of totality $1^m\ 58^s{\cdot}6$, the Sun's altitude

about 18°. For any neighbouring position the beginning and ending of the total phase may be determined from the expressions

$$\cos\omega = -110\cdot2757-[2\cdot08444]\sin l+[1\cdot62804]\cos l\,.\cos(\lambda-160°\ 52'\cdot8)$$
$$t = 8^h\ 58^m\ 13^s\cdot6\mp[1\cdot77303]\sin\,.\,\omega-[3\cdot38763]\sin l$$
$$-[3\cdot85920]\cos l\,.\cos(\lambda-145°\ 34'\cdot2)$$

At Wellington totality begins at $19^h\ 44^m\ 9^s$ local mean time, and continues $1^m\ 31^s$. At Nelson it begins at $19^h\ 37^m\ 22^s$, and continues $1^m\ 11^s$.

The Greenwich time of commencement of the eclipse, near Castle Point, may be found from the expressions

$$\cos\omega = -1\cdot55171-[0\cdot24636]\sin l+[9\cdot74289]\,.\cos l\,.\cos(\lambda-178°\ 48'\cdot8)$$
$$t = 9^h\ 2^m\ 51^s\cdot9-[3\cdot57430]\sin\omega-[3\cdot29927]\sin l$$
$$-[3\cdot82250]\cos\,.\,l\,.\cos(\lambda-161°\ 21'\cdot5)$$

The contact takes place at about 62° N. to W. (direct). For Castle Point the equations give the partial beginning at $18^h\ 45^m\ 47^s$ local mean time. The Sun will be at an altitude of $6\frac{1}{2}°$.

With regard to the latitude limits of total phase, they may be stated approximately at $+49'$ for N. limit and $-50'$ for S. limit, but a closer computation appears unnecessary. For satisfactory observation during the totality of the approaching eclipse, the observer must place himself as nearly as practicable upon the central line. It will be found that the above determination of its course, from my own computations, agrees closely with that indicated in the *American Ephemeris*.

The Observations of the Moon made at the Radcliffe Observatory, during the year 1884, *and a Comparison of the Results with the Tabular Places from Hansen's Lunar Tables.* By E. J. Stone, M.A., F.R.S.

The present paper contains the Right Ascensions and North Polar Distances of the Moon as deduced from the observations made at the Radcliffe Observatory during the year 1884. I have compared these results with those deduced from Hansen's Lunar Tables on two suppositions:—

1. That the mean times, found in the usual way from the sidereal times at mean noon given in the *Nautical Almanac*, were not changed in 1864.

2. That the mean times were changed in 1864, in accordance

with the views which I have explained in papers already communicated to the Society.

The following facts should be borne in mind:—

(1) Hansen's Lunar Tables represented the motions of the Moon with great accuracy for many years before 1863. The mean yearly error, 1847–1864, was always small, generally about $-1''.5$.

(2) In 1864 we changed the scale upon which our mean times are measured, or the ratio of the sidereal day to the tabular day. This is now admitted.

(3) I have given mathematical proofs, which no one in my opinion has shown to be in error, that this change of unit requires the correction

$$24^{h}.\frac{\delta n}{n}.t$$

to be applied to our present sidereal times at mean noon, in order to obtain the sidereal time at Bessel's old mean noon, to which our tabular quantities refer. This correction is equivalent to a change of the adopted unit in the proportion of

$$1 : \left(1 + \frac{\delta n}{n}\right).$$

(4) When this correction is applied to our mean times, the discordances between Hansen's Lunar Tables and observations for the twenty-one years 1864–1884 are sensibly the same as they were before 1863.

The mean of the four greatest negative errors in the comparison of Hansen's Tables in Longitude with the Greenwich observations was $-9''.08$ in 1862, and $-6''.95$ in 1863; whilst the mean of the four greatest positive errors was respectively $+1''.51$ in 1862, and $+3''.52$ in 1863. The Radcliffe observations in 1882, 1883, and 1884, show corresponding mean errors of $-7''.17$, $-6''.60$, and $-8''.01$, and $+3''.93$, $+3''.50$, and $+4''.69$ respectively.

Table I.

Radcliffe Observations of the Moon, 1884.

Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day. 1884.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
29·83	R.	Jan. 10	5 48 51·52	52·28	+0·76	−1·27	−0·51	70 51 51·14	50·29	−0·85	−0·47	−1·32
29·83	R.	11	6 52 7·49	8·26	+0·77	−1·24	−0·47	71 55 30·37	31·84	+1·47	−2·03	−0·56
29·84	F.B.	12	7 53 10·64	11·78	+1·14	−1·19	−0·05					
29·92	F.B.	Feb. 2	1 24 55·80	56·62	+0·82	−1·13	−0·31	80 47 61·08	59·57	−1·51	+5·05	+3·54
29·94	R.	6	5 24 36·72	37·29	+0·57	−1·24	−0·67	70 59 35·88	34·55	−1·33	+0·22	−1·11
29·94	F.B.	7	6 26 22·56	23·47	+0·91	−1·23	−0·32	71 27 27·93	31·64	+3·71	−1·31	+2·40
30·05	R.	Mar. 5	6 7 14·46	15·14	+0·68	−1·22	−0·54	71 22 4·39	6·74	+2·35	−0·80	+1·55
30·05	F.B.	6	7 7 1·98	2 81	+0·83	−1·18	−0·35	72 36 59·51	61·37	+1·86	−2·17	−0·31
30·06	R.	7	8 4 48·20	48·89	+0·69	−1·14	−0·45	74 54 29 72	32·55	+2·83	−3·32	−0·49
30·06	F.B.	8	9 0 13·69	14·51	+0·82	−1·09	−0·27	78 1 50·18	53·99	+3·81	−4·18	−0·37
30·07	R.	10	10 44 35·18	35·90	+0·72	−1·02	−0·30	85 48 27·37	32·25	+4 88	−5·06	−0·18
30·08	R.	12	12 23 12·38	13·26	+0·88	−0·98	−0·10	94 6 36·56	39·22	+2 66	−4·88	−2·22
30·09	R.	14	14 0 19·89	20·85	+0·96	−0·99	−0·03	101 25 2·98	4·80	+1·82	−3·89	−2·07
30·10	R.	18	17 21 53·54	54·66	+1·12	−1·06	+0·06	108 42 34·30	34·40	+0·10	−0·26	−0·16

Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day. 1884.		Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s				h m s	s	s	s	s	° ′ ″	″	″	″	″
30·17	F.B.	Apr.	5	9 36 39·43	40·40	+0·97	−1·06	−0·09	80 34 55·02	58·46	+3·44	−4·54	−1·10
30·19	F.B.		9	12 53 55·01	55·92	+0·91	−0·98	−0·07	96 35 19·78	23·74	+3·96	−4·62	−0·66
30·19	W.		10						100 10 36·37	43·28	+6·91	−4·12	+2·79
30·20	F.B.		12	15 20 47·09	48·43	+1·34	−1·02	+0·32	105 47 7·69	9·81	+2·12	−2·63	−0·51
30·29	F.B.	May	3	10 12 17·67	18·54	+0·87	−1·03	−0·16	83 14 6·57	11·23	+4·66	−4·85	−0·19
30·29	R.		5	11 50 34·99	35·83	+0·84	−0·98	−0·14	91 22 39·19	43·30	+4·11	−4·97	−0·86
30·30	F.B.		6	12 38 25·59	26·54	+0·95	−0·97	−0·02	95 20 45·73	48·69	+2·96	−4·72	−1·76
30·30	R.		7	13 26 17·65	18·35	+0·70	−0·98	−0·28	99 2 22·55	25·21	+2·66	−4·30	−1·64
30·31	F.B.		8	14 14 40·11	41·21	+1·10	−0·99	+0·11	102 18 54·70	57·44	+2·74	−3·70	−0·96
30·31	R.		9	15 3 53·84	54·70	+0·86	−1·01	−0·15	105 2 13·61	15·92	+2·31	−2·94	−0·63
30·31	F.B.		10	15 54 5·04	6·25	+1·21	−1·03	+0·18	107 4 53·91	53·94	+0·03	−2·05	−2·02
30·44	W.	June	10	19 4 34·59	36·06	+1·47	−1·07	+0·40	107 32 45·94	44·63	−1·31	+1·85	+0·54
30·44	F.B.		11	19 56 35·90	37·26	+1·36	−1·06	+0·30	105 37 12·17	10·70	−1·47	+2·87	+1·40
30·53	F.B.	July	2	14 31 19·34	20·42	+1·08	−1·00	+0·08	103 4 17·41	21·34	+3·93	−3·55	+0·38
30·53	W.		3	15 20 39·68	40·72	+1·04	−1·02	+0·02	105 38 5·14	7·77	+2·63	−2·75	−0·12
30·55	R.		7	18 47 15·94	17·01	+1·07	−1·08	−0·01	108 1 61·42	59·33	−2·09	+1·45	−0·64
30·55	W.		8	19 39 53·07	54·24	+1·17	−1·08	+0·09	106 24 61·10	59·30	−1·80	+2·52	+0·72
30·55	F.B.		9	20 32 9·69	10·95	+1·26	−1·07	+0·19	103 57 55·39	51·30	−4·09	+3·49	−0·60

Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day. 1884.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.−Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
30·56	R.	July 11	22 15 26·40	27·57	+1·17	−1·06	+0·11	97 1 15·91	11·86	−4·05	+4·93	+0·88
30·64	W.	31	15 52 16·01	17·01	+1·00	−1·04	−0·04	106 41 6·67	5 36	−1·31	−2·23	−3·54
30·65	R.	Aug. 1	16 43 12·19	13·12	+0·93	−1·06	−0·13	108 5 8·29	7·22	−1·07	−1·23	−2·30
30·65	F.B.	2	17 35 1·63	2·67	+1·04	−1·08	−0·04	108 38 46·00	45·29	−0·71	−0·15	−0·86
30·66	F.B.	4	19 20 23·29	24·31	+1·02	−1·09	−0·07	107 4 15·64	11·88	−3·76	+2·09	−1·67
30·66	W.	5	20 13 15·44	16·50	+1·06	−1·09	−0·03	104 56 54·56	49·80	−4·76	+3·14	−1·62
30·67	F.B.	6	21 5 54·25	55·66	+1·41	−1·08	+0·33	102 1 38·81	34·36	−4·45	+4·05	−0·40
30·67	W.	7	21 58 17·91	19·29	+1·38	−1·08	+0·30	98 26 17·66	13·78	−3·88	+4·78	+0·90
30·76	R.	29	17 14 3·32	4·01	+0·69	−1·07	−0·38	108 20 17·01	16·17	−0·84	−0·61	−1·45
30·77	R.	Sept. 1	19 51 5·45	6·32	+0·87	−1·09	−0·22	105 50 15·58	12·11	−3·47	+2·67	−0·80
30·77	F.B.	2	20 43 53·34	54·36	+1·02	−1·09	−0·07	103 16 26·75	22·16	−4·59	+3·67	−0·92
30·78	W.	4	22 29 47·47	48·43	+0·96	−1·10	−0·14	96 2 35·15	30·54	−4·61	+5·15	+0·54
30·79	R.	5	23 23 15·74	16·74	+1·00	−1·11	−0·11	91 42 38·70	33·78	−4·92	+5·54	+0·62
30·87	W.	27	18 36 9 97	10·95	+0·98	−1·08	−0·10	107 55 15·80	14·33	−1·47	+1·10	−0·37
30·88	R.	29	20 20 6·31	7·11	+0·80	−1·08	−0·28	104 26 58·33	55·21	−3·12	+3·19	+0·07
30·89	W.	30	21 12 19·28	20·25	+0·97	−1·09	−0·12	101 30 53·29	48·02	−5·27	+4·10	−1·17
30·90	R.	Oct. 3	23 52 35·95	36·86	+0·91	−1·14	−0·23	89 15 46·66	43·28	−3·38	+5·68	+2·30
30·90	F.B.	4	0 48 34·51	35·59	+1·08	−1·18	−0·10	84 42 13·26	10·50	−2·76	+5·58	+2·82

Corr. to be subtracted from M.T. computed directly from Sid. Time at Mean Noon from N.A.	Obs.	Day. 1884.	Observed R.A.	Secs. of Hansen's R.A.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in R.A.	Observed N.P.D.	Secs. of Hansen's N.P.D.	Diff. (H.—Obs.)	Corr. due to Error in Time.	Residual Error in N.P.D.
s			h m s	s	s	s	s	° ′ ″	″	″	″	″
30·93	R.	Oct 12	8 53 36·72	37·73	+1·01	−1·16	−0·15	77 21 14·97	18·24	+3·27	−4·06	−0·79
31·00	W.	28	21 40 27·03	27·97	+0·94	−1·07	−0·13	99 41 19·39	15·07	−4·32	+4·45	+0·13
31·00	R.	29	22 32 11·70	12·65	+0·95	−1·09	−0·14	95 51 12·78	9·17	−3·61	+5·10	+1·49
31·02	R.	Nov. 3	3 18 49·66	50·75	+1·09	−1·32	−0·23	74 58 43·71	40·57	−3 14	+3·64	+0·50
31·03	R.	6	6 33 52·30	53·41	+1·11	−1·32	−0·21	71 58 10·62	11·40	+0·78	−1 28	−0·50
31·04	F.B.	7	7 36 24·70	26·18	+1·48	−1·27	+0 21	73 36 12·09	13·56	+1·47	−2·73	−1·26
31·05	R.	9	9 32 7·61	8·67	+1·06	−1·14	−0·08	79 41 10·64	17·48	+6 84	−4·61	+2 23
31·11	R.	24	21 20 22·87	23·55	+0·68	−1·05	−0·37	101 12 43·33	39·68	−3 65	+4·06	+0·41
31·12	R.	28	0 47 13·18	14·02	+0·84	−1·15	−0·31	84 56 58 68	54·24	−4·44	+5·55	+1·11
31·13	F.B.	29	1 44 7·08	8·12	+1·04	−1·22	−0·18	80 37 60·26	57 13	−3·13	+5·18	+2·05
31·23	W.	Dec. 26	1 18 24·29	25·37	+1·08	−1·15	−0 07	82 45 44·47	41·96	−2·51	+5·25	+2·74
31·25	W.	30	5 24 15·12	15·97	+0·85	−1·39	−0·54	71 34 28·72	27·91	−0·81	+0·69	−0·12
31·25	F.B.	31	6 31 26·47	27·48	+1 01	−1·39	−0 38	71 45 3·37	4·81	+1·44	−1·15	+0·29
	Mean of Errors without regard to sign ...				0·989		0·207			2·907		1·140
	Mean Errors for Year				+0·989		−0·120			−0·270		−0·094

Observers.—W., Mr. Wickham; R., Mr. Robinson; F.B., Mr. Bellamy.

TABLE II.

Radcliffe Observations of the Moon, 1884.

Errors of Longitude and Ecliptic Polar Distance. Corrected and Uncorrected for Error in Mean Time.

Day, 1884.	Errors of Longitude. *Corrected* for Error in Time.	*Uncorrected.*	Errors of E.N.P.D. *Corrected* for Error in Time.	*Uncorrected.*
	″	″	″	″
Jan. 10	−7·22	+10·82	−1·47	−0·62
11	−6·75	+11·11	+0·04	+0·48
Feb. 2	−5·58	+11·84	+1·58	+3·12
6	−9·44	+ 8·17	−1·70	−0·82
7	−4·46	+13·14	+2·60	+3·12
Mar. 5	−7·69	+ 9·74	+1·66	+2·21
6	−5·03	+12 06	+0 28	+0·46
7	−6·50	+10·41	+0·85	+0·72
8	−3·92	+12·67	+0·77	+0·25
10	−4·23	+11·85	+1·52	+0·05
12	−2·25	+13·14	−1·44	−2·78
14	−1·13	+13·88	−1·79	−3·16
18	+0·84	+15·96	−0·22	−0·95
Apr. 5	−1·62	+14·75	−0·61	−1·38
9	−1·22	+14·05	−0·21	−1·59
12	+4·34	+19·27	−1·68	−2·91
May 3	−2·30	+13·81	+0·67	−0·26
5	−2·27	+13·22	+0·05	−1·24
6	−0·97	+14·20	−1·50	−2·85
7	−4·46	+10·62	+0·02	−1·38
8	+1·20	+16·13	−1·44	−2·77
9	−2·26	+12·62	0·00	−1·23
10	+2·11	+17·01	−2·52	−3·59
June 10	+5·65	+21·12	+1·17	+1·02
11	+4·00	+19·65	+2·21	+2·37
July 2	+1·23	+16·22	−0·01	−1·23
3	+0·25	+15·23	−0·19	−1·31
7	−0·09	+15·45	−0·65	−0·81
8	+1·16	+16·94	+0·93	+1·06
9	+2·84	+18·85	+0·10	+0·54
11	+1·22	+17·76	+1·41	+2·48

Day, 1884.	Errors of Longitude. *Corrected* for Error in Time.	Errors of Longitude. *Uncorrected.*	Errors of E.N.P.D. *Corrected* for Error in Time.	Errors of E.N.P.D. *Uncorrected.*
	"	"	"	"
July 31	−1·31	+13·79	−3·34	−4·32
Aug. 1	−2·15	+13·06	−2·04	−2·80
2	−0·61	+14·81	−0·83	−1·33
4	−0·77	+15·06	−1·79	−1·73
5	−0·07	+16·09	−1·68	−1·25
6	+4·76	+21·15	+1·02	+1·73
7	+3·88	+20·61	+2·37	+3·40
29	−5·52	+ 9·75	−1·02	−1·62
Sept. 1	−2·98	+13·03	−1·38	−1·06
2	−0·75	+15·63	−1·16	−0·53
4	−2·14	+15·04	−0·27	+1·00
5	−1·76	+15·75	−0·08	+1·37
27	−1·41	+14·11	−0·46	−0·60
29	−3·99	+12·07	−0·86	−0·38
30	−1·34	+15·23	−1·64	−0·80
Oct. 3	−4·08	+13·88	+0·74	+2·33
4	−2·48	+15·94	+2·01	+3·74
12	−2·34	+15·17	−0·16	−0·91
28	−1·86	+14·59	−0·50	+0·45
29	−2·50	+14·53	+0·61	+1·89
Nov. 3	−3·35	+16·09	−0·38	+1·06
6	−3·03	+15·92	−0·32	−0·15
7	+2·78	+21·32	−1·73	−2·01
9	−0·41	+17·06	+2·49	+1·50
24	−5·32	+10·66	−1·27	−0·42
28	−4·70	+13·29	−0·78	+0·80
29	−3·22	+15·50	+0·96	+2·59
Dec. 26	−2·00	+15·85	+2·15	+3·70
30	−7·68	+12·16	−0·60	−0·05
31	−5·41	+14·49	+0·59	+0·64
Mean of errors without regard to sign	3·096	14·645	1·091	1·556
Mean errors for year	−1·907	+14·645	−0·146	−0·111

TABLE III.

Observations of the Moon, 1862 to 1884.

Mean Errors of Longitude. Uncorrected and Corrected for Error in Mean Time.

Year.	Errors of Longitude. (Hansen − Observed). *Uncorrected.*	*Corrected.*
1862 Greenwich	− 2″·829	−2″·829
1863	− 1·606	−1·606 *
1864	+ 0·121	−0·814
1865	+ 1·271	−0·220
1866	+ 2·142	−0·217
1867	+ 3·480	+0·357
1868	+ 4·117	+0·280
1869	+ 4·277	−0·352
1870	+ 4·828	−0·657
1871	+ 6·955	+0·435
1872	+ 7·309	+0·097
1873	+ 8·239	+0·200
1874	+ 9·294	+0·561
1875	+ 9·867	+0·365
1876	+ 9·800	−0·509
1877	+ 9·234	−1·898
1878	+ 8·219	−3·603
1879	+ 9·631	−3·124
1880	+10·265	−3·245
1881	+10·622	−3·791
1882 Radcliffe	+12·927	−2·508
1883 „	+14·615	−1·547
1884 „	+14·645	−1·907

On Screw-wear as affecting the N.P.D. of the Cape Catalogue for 1880. By E. J. Stone, M.A., F.R.S.

The statement which Mr. Gill has made of the existence of serious systematic errors in the North Polar Distances of the Cape Catalogue for 1880, due to the wear of the screws of the Transit Circle between 1856 and 1879, is one which is easily made; but which it will be found much more difficult to prove than to make.

* Here change in the unit of time took place.

That two surfaces which rub must to some extent wear is true; but that the relative wear over the five or six threads which are in continuous use in Observatory work is sufficient to lead to serious errors in the results is a question which can only be settled by a direct appeal to facts.

Fortunately, whilst at the Cape, I did directly examine this question. It was, and still is, my rule to test, from time to time, the working of the screws, as a whole, by observations of a fixed collimating mark, or, better still, by Nadir Point Determinations at different parts of the screws. If it can be shown that the screws will allow a direction to be accurately determined at any of the threads used in the work, then it is needless, for all practical purposes, to discuss further questions of screw-wear; and if any errors due to screw-wear or defects in the screws affect the results, such experiments as I have described will clearly indicate their magnitude.

The results of observations of the Nadir at 0^r and 2^r made in 1875 are printed in the Cape Observations for 1875; but in 1877, in consequence of discussions which had taken place in England with regard to screw-wear, observations of the Nadir direction were made over the whole range of the threads of the screws which *could* possibly be used in making observations, including some that no careful observer would think of using from the mere feel of the working of the screw. The result was to show that, so long as the observations were confined to the five or six middle threads of the screws which must be employed in passing from one division of the circle to the next, and which were constantly in use, the errors due to defect of the screws of the Cape instrument were restricted within very small limits indeed; but that the employment of one or two threads outside this necessary range soon brought relative errors into existence. The curve, which Mr. Gill has given, is the reproduction of one which I constructed from these observations in 1877. It shows clearly the smallness of the errors so long as the central threads are alone used; and the extreme rapidity with which the errors increase as the readings are unduly extended. But an inspection of the observations shows that no continuous curve can accurately represent the observations, the fact to which they point is a *per saltum* change at or about 5^r. Whether this rapid increase of error be due to constraint or not, the fact of the rapid increase of relative error when these outside threads are engaged is undoubted. Such rapidity of increase of the errors when the extreme threads of the screw are brought into play shows that it would be useless to compare errors of the screws at different times from a few existing readings, unless it were perfectly certain that no changes whatever had been made in the adjustment of the index from which the revolutions are taken. It would be quite possible that the screws should be working without error from 0^r to 5^r at one time, and that no inconsiderable errors should arise from the use of the same readings at another if the

index has been adjusted relatively to the screws. As a matter of fact such adjustments are made from time to time. This probably explains why Mr. Gill has come to a different conclusion to myself with respect to the existence of strained readings when the screws were new. I have not the records before me, but I know that I carefully looked into the matter at the time; but as the strain becomes important only when extreme readings are used, it is impossible for me now to say whether such erroneous readings began at 0^r, or -1^r, or -2^r; all I assert is, that within the limits of range of screws occasionally, but, I presume, accidentally, used, there were some indications in 1856–1860 of exactly the same character as that shown by the curve resulting from the observations made in 1877. In exactly the same way there is visible wear in the screws of the Radcliffe Transit Circle, but experiments made over the threads in constant use give such results as those which follow, which include errors of screws and of observations:—

1884, July 10.

Reading of the Declination Micrometer.	Resulting Nadir Point.	Correction to Mean.
15·1	218 26′ 4″·28	+0″·15
16·1	4·47	−0·04
17·05	4·64	−0·21
18·3	4·34	+0·09
19·1	4·48	−0·05
20·3	4·26	+0·17
21·1	4·27	+0·16
22·2	4·56	−0·13
23·3	4·37	+0·06
24·1	4·65	−0·22
25·2	4·44	−0·01

1^r of the micrometer = 32″·045.

Similar results have been obtained in other yearly examinations. The index has been adjusted, so that if negative readings be not taken, the errors are confined within the limits indicated by these observations, which are quantities within which I cannot answer for our work. The assistants are instructed, as they were at the Cape, to carefully avoid negative readings, and I can hardly understand how Mr. Gill can have persuaded himself that screws worn only to the extent indicated by the Cape Nadir observations in 1877 could have led to serious systematic errors in the catalogued results. That such is quite impossible can be seen by an inspection of his Tables VII. and VIII., when it is remembered that the observations for Nadir, runs, and of stars were confined within the limits adopted in the Cape work. But it is

quite possible to give a perfectly independent proof of the non-existence of any serious systematic errors due to screw-wear.

The Cape Transit Circle was brought into regular use in 1856; and during the years 1856 to 1860 a very considerable number of observations were made of the stars which have been regularly observed at Greenwich since Bradley's time. These observations were reduced whilst I was at the Cape, and are contained in the Cape Catalogue for 1860. The N.P.D. of the Cape Catalogue for 1860 can be assumed to be independent of any serious errors due to the wear of the screws which were at that time new. Since the proper motions of these stars can be determined with some approach to accuracy from observations perfectly independent of the Cape results, we can bring up the N.P.D.'s from 1860 to 1880 with some accuracy without in the slightest degree diminishing any systematic discordances which may exist between the N.P.D.'s of the two Cape Catalogues. I have therefore selected from the Cape Catalogues 1860 and 1880 all the stars whose N.P.D.'s are fixed by at least ten observations in the 1860 Catalogue, and three observations in the 1880 Catalogue, and which have the proper motions in N.P.D., referred to the adopted precession constant, determinable with some approach to accuracy from a comparison between Bradley's observations, or in two cases Piazzi's, and recent Greenwich observations. The N.P.D. of the Cape Catalogue for 1860 are brought up to 1880 with Peters' constant of precession and the adopted proper motions, and the differences taken between these results and those contained in the Cape Catalogue for 1880.

The differences will be affected by at least three independent sources of error.

(1) There will be the error of the N.P.D. given in the Cape Catalogue 1860; I shall certainly not over-estimate the probable error due to this cause by taking

$$e_{1860} = 0''.1.$$

(2) There will be the combined effects of error in the precession constant and adopted proper motion for twenty years. As this cannot be less than $0''.1$, and will be about one-sixth of that due to the relative errors of Bradley's N.P.D. and the recent Greenwich observations, I cannot over estimate the probable error by taking

$$e_m = 0''.2.$$

(3) There will be the probable error of the N.P.D. of the Cape Catalogue of 1880. As there are generally only three or four observations employed in fixing the N.P.D., the probable error, independently of any possible error due to screw wear, will be about

$$e_{1880} = 0''.25.$$

We shall therefore have for the probable error of the computed differences 1860—1880 on the supposition that there are no systematic errors due to screw-wear

$$\overline{pe} = \sqrt{\overline{0\cdot1}^2 + \overline{0\cdot2}^2 + \overline{0\cdot25}^2} = 0''\cdot33.$$

But as these screw-errors, if they exist, must increase the probable error due to mere ordinary chance errors of observing, we shall have, when the probable error due to the screw-wear is taken $=x$

$$\overline{pe} = \sqrt{\overline{0\cdot1}^2 + \overline{0\cdot2}^2 + \overline{0\cdot25}^2 + x^2}.$$

If, therefore, x could be taken $= 1''$, we should have

$$\overline{pe} = 1''\cdot055.$$

If x could be taken $= 0''\cdot5$

$$\overline{pe} = 0''\cdot60.$$

If Mr. Gill does not mean to assert that the Cape N.P.D's are affected by error to $0''\cdot5$ from the cause indicated, then I think his paper should not have been written: but even if x could be taken at $0''\cdot25$ we should still have

$$\overline{pe} = 0''\cdot42.$$

Now a direct computation of the probable error from the observed differences gives

$$\overline{pe} = 0''\cdot28.$$

This would show that x is really insensible, and that no serious errors can have affected the N.P.D. observations contained in the Cape Catalogue for 1880 due to any such cause as screw-wear.

I append the comparisons between the two Catalogues from which the probable error above given has been deduced.

The agreement between the results, allowing for the few observations by which the N.P.D. of the 1880 Catalogue were fixed, is exceedingly good. There are only two cases out of the 92 comparisons in which the difference between the two Catalogues amounts to a second of arc. The mean excess of the N.P.D. of the 1880 Catalogue is only $+0''\cdot06$, the range of N.P.D. being between 51° and 123°, whilst the co-latitude determined from the observations 1856–1860 was

$$123^\circ\ 56'\ 3''\cdot56,$$

that found from the more recent observations and included in the Cape Catalogue 1880, was

$$123^\circ\ 56'\ 3''\cdot41.$$

I have in the Introduction to the 1880 Catalogue given comparisons between the Cape and Greenwich results, the mean discordance being 0″·31.

There does not, therefore, appear any such indication of constant error as might be inferred from Mr. Gill's remarks.

I hope that Mr. Gill will be better advised than to apply empirical corrections to the North Polar Distances of the Cape Catalogue for 1880. It is quite impossible for him to obtain the true corrections due to any errors which may have existed. The runs are an essential part of the reductions. If the screws had unfortunately been sufficiently worn to prevent reliable readings being taken at any part of the threads in regular use, then most certainly the threads in use for the run determinations could not have been trusted. But as proved from the experiments made during the progress of the work, the effects of screw-wear are so small that the systematic errors due to this cause are certainly less than the probable errors of mere observations in the catalogued results.

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D. −	Residual Errors N.P.D. +	No. of Obs. in Cape Cat. 1860.	No. of Obs. in Cape Cat. 1880.
	″	″	″		
γ Pegasi	+0·011	−0·41		33	7
12 Ceti	+0·012		+0·48	13	9
β Ceti	−0·025	−0·10		122	3
δ Piscium	+0·048		+0·01	12	3
ϵ Piscium	−0·023		+0·11	34	5
θ Ceti	+0·220		+0·39	42	4
η Piscium	+0·007		+0·43	24	5
ν Piscium	−0·002		+0·03	16	4
β Arietis	+0·118		+0·25	30	3
α Arietis	+0·151	−0·24		38	5
67 Ceti	+0·110		+0·02	28	4
ξ^2 Ceti	+0·016		+0·32	13	4
α Ceti	+0·086	−0·16		31	5
δ Arietis	+0·002		+0·36	33	3
γ′ Eridani	+0·096		+0·37	30	4
o′ Eridani	−0·080		+0·94	29	3
α Tauri	+0·192	−0·06		103	4
ϵ Leporis	+0·072	−0·16		26	7
β Orionis	+0·006		+0·09	109	5
β Tauri	+0 181	−0·29		35	3
δ Orionis	+0·005		+0·14	35	5
α Columbæ	+0·045	−0·21		39	7

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D. −	Residual Errors N.P.D. +	No. of Obs. in Cape Cat. 1860.	No. of Obs. in Cape Cat. 1880.
	″	″	″		
α Orionis	−0·007	−0·17		90	3
ζ Canis Maj.	+0·005		+0·16	10	3
γ Geminor.	+0·048	−0·13		17	3
ε Canis Maj.	+0·013	−0·32		70	4
δ Geminor.	+0·016		+0·52	31	3
6 Cancri	+0·036		+0·79	11	6
15 Argûs	−0·059	−0·42		49	6
η Cancri	+0·054		+0·35	27	3
ε Hydræ	+0·053	−0·09		19	3
α Hydræ	−0·030		+0·27	67	6
π Leonis	+0·022		+0·65	27	5
α Leonis	−0·012	−0·31		88	3
χ Leonis	+0·043	−1·10		43	3
δ Crateris	−0·183	−0·19		71	3
τ Leonis	+0·021		+0·73	16	3
υ Leonis	−0·035		+0·16	22	3
β Leonis	+0·119	−0·21		14	3
ε Corvi	−0·020	−0·36		19	3
β Corvi	+0·066	−0·30		98	3
θ Virginis	+0·041		+0·54	26	7
α Virginis	+0·038		+0·35	118	4
ζ Virginis	−0·059		+0·80	11	4
α Boötis	+1·977	−0·27		37	4
20 Libræ	+0·032		+0·04	15	3
β Libræ	+0·031		+0·35	42	3
α Serpentis	−0·034	−0·26		17	3
β Scorpii	+0·038		+0·01	77	4
δ Ophiuchi	+0·143		+0·49	24	4
σ Scorpii	+0·026	−0·47		14	3
α Scorpii	+0·037	−0·45		139	13
τ Scorpii	+0·043	−0·63		20	3
θ Ophiuchi	+0·029	−0·52		53	4
α Ophiuchi	+0·218	−0·34		20	3
Stone 9795	+0·039		+0·22	11	3
μ Sagittarii	+0·012	−0·36		72	3
δ Sagittarii	+0·027		+0·14	29	3
ε Sagittarii	+0·130		+0·61	19	4
λ Sagittarii	+0·224	−0·36		20	5

Name of Star.	Proper Motion in N.P.D.	Residual Errors N.P.D. −	Residual Errors N.P.D. +	No. of Obs. in Cape Cat. 1860.	No of Obs. in Cape Cat. 1880.
	″	″	″		
α Lyræ	−0·272		+0·06	16	3
ϕ Sagittarii	+0·008	−0·53		24	3
σ Sagittarii	+0·077	−0·02		40	3
ζ Sagittarii	+0·006	−0·49		23	3
τ Sagittarii	+0·251	−0·08		18	3
ψ Sagittarii	+0·029	−0·28		14	4
ω Aquilæ	−0·022		+0·41	24	3
δ Aquilæ	−0·076	−0·02		36	5
l^2 Sagittarii	+0·027		+0·19	46	3
γ Aquilæ	+0·008		+0·13	12	4
α Aquilæ	−0·380	−0·95		108	5
b Sagittarii	+0·036		+0·05	13	3
c Sagittarii	−0·016	−0·26		21	5
α^2 Capricorni	0·000		+0·85	86	3
ρ Capricorni	+0·020	−0·08		46	4
ψ Capricorni	+0·162	−0·16		15	4
θ Capricorni	+0·075		+0·39	12	3
ι Capricorni	−0·004		+0·41	16	3
β Aquarii	+0·016		+0·27	95	7
ϵ Pegasi	+0·006		+0·33	21	3
δ Capricorni	+0·313		+0·13	21	3
α Aquarii	+0·014	−0·20		33	10
θ Aquarii	+0·025		+0·62	35	3
σ Aquarii	+0·030		+1·08	12	5
η Aquarii	+0·040		+0·27	22	7
γ Piscis Aust.	(0·000)		(+0·01)	10	3
α Piscis Aust.	+0·171		+0·26	123	6
ϕ Aquarii	+0·177		+0·33	10	3
γ Piscium	−0·009		+0·31	26	3
κ Piscium	+0·111	−0·08		36	9
ι Piscium	+0·443		+0·66	23	9
δ Sculptoris	+0·103		+0·02	16	3
ω Piscium	+0·104	−0·20		27	7

On account of the variability in their proper motions I have not included *Sirius* and *Procyon*.

It is perhaps desirable that some notice should be taken of the statement which Mr. Gill has made in the Introduction to the Cape Catalogue for 1850 with respect to the differences

between the Right Ascensions of this Catalogue and those of the Cape Catalogue for 1880 when reduced to a common epoch. Mr. Gill appears to consider that these differences indicate systematic errors in the Cape Catalogue for 1880 due to variation in the place of collimation. I must remark, however, that materials do not exist for an accurate determination of the proper motions of the southern stars with reference to the adopted precession constant; and we cannot bring up places for a period of thirty, or even twenty, years with sufficient accuracy to justify attempts to base discussions of instrumental systematic errors upon differences of observed places when thus compared. Such differences may, of course, be due to instrumental defects, but they may be principally due to uncorrected proper motion. But the mean difference found by Mr. Gill of $0^s.06$ divided by 30 years gives only a yearly change of $0^s.002$, and this is within the possible, and I should suppose probable, errors of the determination of our precession constant. I have compared the Melbourne Right Ascensions made in 1875 with the Cape Observations 1870–1880; and in this comparison we are not much concerned with proper motions. The result, which I find appears clearly to indicate that the differences between the Cape Catalogues 1850 and 1880, are not due to either lateral refraction or any systematic variation in the plane of collimation of the Cape Transit Circle, unless the Melbourne instrument is affected by sensibly equal errors. The Melbourne clock errors are apparently based on the Greenwich standard places, and as the Cape results are also based on the same places, the Right Ascensions of the clock stars must therefore agree, and I have not included their differences in the following result. But between 124° and 170° N.P.D. I have found 46 stars which were observed at Melbourne in 1875, and whose Right Ascensions are contained in the Cape Catalogue 1880. Instead of finding the Cape R.A. too small, as would appear to be the case from Mr. Gill's comparisons with the Cape Catalogue for 1850, I find the difference reaches the limit of two-tenths only in one case, when a single observation only was used for Melbourne R.A., and that the mean excess of the R.A. Cape 1880, over R.A. Melbourne 1875, is only $+0^s.02$. I do not wish to assert that the Cape results are free from systematic errors, but I am certain that if such exist they are small, and that such systematic errors in Right Ascension as may exist will be chiefly dependent upon the adopted Right Ascensions of the clock stars.

Addendum.

There are some statements of Mr. Gill's on pages 80 and 81 to which, on seeing his paper in print, it appears desirable that I should make some reply.

1. The screw constraint was not due to dirt. It was present, whether the screws were clean and fresh oiled or not, when certain threads were brought into play.

2. "Occasional notes such as 'screws turned stiffly,' followed soon after by a note that 'the screws were cleaned,' and there are no more notes about stiffness of the screws for a long time," merely show that the Cape screws, like other screws, occasionally required cleaning, and were cleaned.

3. Mr. Gill's attempted proof of progressive wear between 1856–57 and 1872–73 consists merely in showing that the runs were about $0^{r}.014$ greater in 1872–73 than in 1856–57, and an *assumption* that there were no adjustments of the microscopes between those dates. If Mr. Gill had given the runs after June 1873, he could have proved, on the same assumptions, that the screws had worn in the opposite direction; for the runs after June 1873 are about $0^{r}.028$ *less* than those in 1857–58. Mr. Gill must have seen that such changes are principally due to adjustments; and he might have known that as the microscopes A and B had to be taken off in 1873 to allow the supplementary microscopes *a* and *b* to be dismounted, as is stated in the Cape Results, 1873, page xvi.; and as these microscopes *a* and *b* had been mounted by me soon after my arrival at the Cape, 1870–71, in order to determine the division-errors of the circle, the microscopes A and B must, at that time, have also been dismounted in order to mount the microscopes *a* and *b*. If, therefore, we suppose that the adjustments of the microscopes had not been disturbed after 1857–58 till I arrived at the Cape, a most improbable assumption, they certainly had been disturbed before the readings 1872–73.

4. An inspection of the observations made by me in 1877 shows clearly enough that no continuous curve can be drawn to represent the errors of the screws; the observations between $5^{r}.1$ and $5^{r}.8$ fall on a straight line, and there is less discordance than $0''.5$ between $5^{r}.1$ and $10^{r}.1$. The fact brought out by these observations is that the errors changed *per saltum* when the parts of the screws which corresponded to the readings about my 0^{r}, or Mr. Gill's 5^{r}, were brought into play. If, therefore, it was wished to make accurate observations with these screws, it could be done by avoiding the use of these threads about 5^{r}, and in no other way, and this is the course which I have followed. This is clearly shown by Mr. Gill's extracts from my notes, for it will be seen that even in 1872–73 negative readings were most carefully avoided. The indices of the microscopes were adjusted so that the defective threads of the screws could not be employed in the observations so long as negative readings were avoided; and the assistants were cautioned against the use of negative readings. If I found that such readings for runs were accidentally made, they were no doubt rejected because it was known that they must be defective, and ought not to have been made; and if at any time I had reason to suppose that the defective threads were from slight changes again coming into use, the indices were readjusted to avoid the further use of these defective threads. An inspection of the results of the

observations made in 1877 shows that with these precautions the effects of screw errors must have been confined within very small limits even in individual observations, and in the results of three or four observations must be practically insensible, and this result is confirmed by the comparison which I have given between the Cape Catalogue 1860 and the Cape Catalogue 1880. The larger errors which appear in Mr. Gill's Tables VII. and VIII. have nothing to do with the systematic errors of the Cape Catalogue 1880, for the simple reason that the threads of the screws to which they apply were not used in the work.

Mr. Gill's idea appears to be that I was bound to make observations over the whole range of the screws, whether the threads were defective or the screws under constraint or not, and then to attempt to determine the errors of these screws and to apply corrections to the results. I preferred to avoid the large errors altogether, and I have done so. Every attempt to carry out Mr. Gill's ideas into practice with the Cape screws would have ended in a disastrous failure. The change of error is so rapid about the 0^r (or Mr. Gill's 5^r) that the errors of the screws could not be determined with any great accuracy, or applied with any certainty subject to such shifts of adjustments as are inevitable in practical work. But whether Mr. Gill's ideas might have been practically carried out or not, I was certainly entitled to carry out my own ideas, which I knew would lead to results of very considerable accuracy. I have carried them out, and I am satisfied with the result.

Note on the Descriptions of two Stars in Ptolemy's Catalogue.
By E. B. Knobel.

In the first printed edition of the *Almagest*, which is that published in Latin by Liechtenstein at Venice in 1515, the descriptions of the 8th star in *Scorpio* (*α Scorpii*) and the 2nd star in *Orion* (*α Orionis*) are as follows:—

8. *Scorpio.* "Media earum quæ *tendit ad rapinam* quæ dicitur Cor Scorpionis."
2. *Orion.* "Lucida quæ est super humerum dextrum et ipsa *tendit ad rapinam* quia * appropinquat ad terram in humero Orionis."

Baily remarks in his compiled edition of *Ptolemy's Catalogue*, "There is a singular expression in the edition of Liechtenstein that I am unable to explain. It first occurs in the constellation *Scorpio*, and is repeated in the constellation *Orion*, where the star is described as "tendens ad rapinam."

It is well known that the Liechtenstein *Almagest* is derived

* Baily translates the abbreviation in Liechtenstein as "quæ," but Mr. Scott, Assistant-Keeper of MSS. British Museum, tells me it is undoubtedly "quia."

from Arabic sources. There is abundant internal evidence to prove this; notably in Dictio VII. cap. i., where Arabic names are given to stars, which in the Greek *Almagest* are without names; the proper name Hipparchus is spelt "Abrachis," after the manner of the Arabs, and in the Catalogue many Arabic names are given to the stars and constellations.

I have, however, recently made a close comparison between this printed edition and three Latin MS. *Almagests* of the fourteenth and fifteenth centuries, which are copies of the translation from the Arabic by Gerard of Cremona (A.D. 1114–1187); and from the numerous passages compared being verbatim the same, and particularly from the same blunders and mistakes being common to them all, I think there can be no doubt whatever that the Liechtenstein *Almagest* is simply the printed edition of Gerard of Cremona's translation. Weidler makes the following remark about it. "Sed interpretis nomen non adscribitur nec in præfatione et epilogo memoratur. Collata vero utraque versione apparet hanc Venetam ex Arabico traductam fuisse, et a Trapezuntiana permultum differre."*

The MSS. referred to are the following:—

British Museum, Sloane MS., 2795. Date, circa 1300, possibly earlier.
British Museum, Burney MS., 275. Date, circa 1387.
Latin MS., No. 148–9, belonging to the Earl of Crawford and Balcarres. Date, sæc. XV.

The descriptions of the two stars in question in these three MSS. and the Liechtenstein *Almagest*, are verbatim the same, with the exception that the printed edition alone adds to one of them the words "quia appropinquat ad terram in humero Orionis."

Thinking that some light might possibly be thrown upon the meaning of the sentence "tendens ad rapinam" by examining the original Arabic, I have consulted and obtained extracts from four different Arabic MSS. found in the British Museum, the Bodleian Library, and the Library of the India Office. These MSS. are:—

British Museum, Additional MSS., 7475. *Almagest*, date A.H. 615=A.D. 1218. This MS. is written in a very cursive character, with a lamentable neglect of diacritical points, which renders it very difficult to decipher.
Bodleian Library, Pococke 369 (Uri 888). *Almagest*, date A.H. 799=A.D. 1396.
British Museum, Additional MSS., 7488. "Al Sūfi, Description of the Stars." Date, sæc. XV. or XVI. A fairly well written MS., with very good drawings of the constellations.
India Office Library, No. 2389. "Al Sūfi, Description of the Stars."

* Baily says "the name of the translator of the Liechtenstein *Almagest* not known, nor is it stated whence the MS. was obtained."

The last-named MS. has been kindly examined for me by Dr. J. W. Redhouse, who has taken great trouble to elucidate and solve a difficulty in the translation.

The following descriptions of these stars comprise all the original MSS. and works I have been able to examine:—

8th Star in Scorpio.

1. Greek *Almagest.*

ὁ μέσος αὐτῶν καὶ ὑπόκιρρος καλούμενος ἀντάρης.

2. Latin *Almagest* from Greek (Trapezuntius).

Media ipsarum et subruffa quæ vocatur Antares.

3. Latin *Almagest* from Arabic (Gerard of Cremona).

Media earum quæ tendit ad rapinam quæ dicitur Cor Scorpionis.

4. Arabic *Almagest.* Brit. Mus. Add. MSS., 7475.

الكوكب الوسط منها و هو قلب العقرب و هو شمعى و يسمي بالرومي انطرس

The middle star of them, and it is *Cor Scorpionis*, and it is wax-like, and is named in Greek *Antares.*

5. Arabic *Almagest.* Bibl. Bodl. Pococke, 369.

الوسط منها الذي الحوصي و هو قلب العقرب

The middle one of them, which (is) ? and it is *Cor Scorpionis.*

6. Al Sūfi. Arabic MS. Brit. Mus. Add. MSS., 7488.

الوسط منها الذي يضرب الي الحوض و يقال لم قلب العقرب

The middle one of them, which inclines to ? and is named *Cor Scorpionis.*

7. Al Sūfi. Arabic MS. India Office, No. 2389.

الوسط منها الذي يضرب الي الحرض و يقال له قلب العقرب

(Nearly identical with Brit. Mus. MS.)

8. Al Sūfi. Schjellerup's translation.

Leur mitoyenne qui tire sur le rouge يضرب الي الخوصي nommée قلب العقرب *le cœur du Scorpion.*

9. Ulugh Beigh. Hyde's translation from Persian.

Media trium quæ magna est ac rufa. Kalb Al Akrab.

2nd Star in Orion.

1. Greek *Almagest.*

ὁ ἐπὶ τοῦ δεξιοῦ ὤμου λαμπρὸς ὑπόκιρρος.

2. Latin *Almagest* from Greek (Trapezuntius).

Splendida quæ in humero dextro et est subruffa.

3. Latin *Almagest*, from Arabic (Gerard of Cremona).
"Lucida quæ est super humerum dextrum et ipsa tendit ad rapinam." Liechtenstein's printed edition adds, "quia appropinquat ad terram in humero Orionis."

4. Arabic *Almagest*. Brit. Mus. Add. MSS. 7475.
الكوكب المضيء الشمعي الذي على المنكب اليمن
The bright star wax-like, which is upon the right shoulder.

5. Arabic *Almagest*. Bibl. Bodl., Pococke, 369.
النير الذي على المنكب اليمن و هو يضرر * الي الحوصي
* (Copyist's error for يضرب)
(Identical with Al Sūfi.)

6. Al Sūfi. Arabic MS. Brit. Mus. Add. MSS. 7488.
النير الذي على المنكب الايمن و هو يضرب الي الحوض
The bright (star) which is on the right shoulder inclining to ?

7. Al Sūfi. Arabic MS. India Office.
النير الذي على المنكب اليمن و هو يضرب الي الحوض
(Identical with Brit. Mus. MS.)

8. Al Sūfi, Schjellerup's translation.
La brillante qui est sur l'épaule droite et qui tire sur le rouge يضرب الي الخوصي

9. Ulugh Beigh. Hyde's translation.
Stella lucida in humero dextro quæ ad rubedinem vergit.

All these descriptions, with the exception of No. 3 by Gerard of Cremona, contain some word which is intended, probably, as a translation of the Greek ὑπόκιρρος, to express the *colour* of the star.

But there is a marked and singular difference in the Arabic authorities.

In the British Museum *Almagest* the four principal red stars of Ptolemy, *α Tauri*, *β Geminorum*, *α Scorpii*, and *α Orionis*, described by Ptolemy as ὑπόκιρρος, are designated شمعي (shamāie), which means "like wax," from شمع (wax).* This is a satisfactory explanation of the enigmatical words "et est cerea," which Liechtenstein uses in the descriptions of the 14th star of *Taurus* (*α Tauri*), and the 2nd star in *Gemini* (*β Geminorum*), and which have been always a puzzle to students of the *Almagest*.

In the Bodleian *Almagest*, however, the corresponding word

* It should be noted that the Bodleian *Almagest* describes the colour of three of these stars as well as *Sirius*, by the sentence يضرب الي الحوصي

is الحوصي, and in this MS. and the two MSS. of Al Sūfi as well as in Schjellerup's translation we have the following variations:—

الحمرة	الحرض	الحوض	الخوصي	الحوصي
El harah.	El harad.	El hūd.	El chūsi.	El hūsi.

The general resemblance of all these words suggests errors in copying.

The word الحوصي, el hūsi, in the Bodleian *Almagest*, has no meaning, and must be an error of the scribe. It is not found in any of the other MSS.

Prof. Schjellerup, in his Al Sūfi, gives Ptolemy's correct description to the stars in question, and also to the red star *α Tauri*, but he appends the Arabic word الخوصي, which he translates "rouge," or "rougeâtre." Dr. Rieu, the Keeper of Oriental MSS. at the British Museum, and Dr. Redhouse inform me there is no such Arabic word implying colour. In fact, Prof. Schjellerup translates the word as it ought to be, his Arabic MS. having an untranslatable word.

In the British Museum MS. of Al Sūfi the word ὑπόκιρρος in *β Geminorum* and *Antares* is in each case translated الحوض, el hūd, and the same word is used in the India Office MS. in the descriptions of the 2nd star in *Gemini* and the 2nd in *Orion*. There appears to be no known meaning to this word implying colour.

Dr. Redhouse considers that the word الحرض, el harad, which he finds in the description of the 8th star in *Scorpio* (and which I have also found in the British Museum MS. of Al Sūfi in the description of the 2nd star in *Orion*), is the real word, of which the others are only copyists' errors. He gives the meaning of الحرض, el harad, as "sallowness of complexion, pallor," which is in fair agreement with ὑπόκιρρος, "somewhat yellow;" and also with the expression "et est cerea," "wax-like;" the identical word in Arabic for which شمعي is found in the British Museum *Almagest*.

This explanation may be the true one, but there is the fifth word, الحمرة, el harah, which I find in the British Museum MS. of Al Sūfi in the description of the 14th star in *Taurus*, and which Dr. Redhouse finds also used for the same star in the India Office MS. He infers that this is meant for الحمرة, el hamra, "redness." The word الحمرة is derived from حمر, meaning "heat," "fierce," "fiery," and this meaning is clearly applicable to the stars in question. It is quite obvious that all the words are intended to express the colour of the star.

It is by no means clear what the word was which Gerard of

Cremona translated as "rapina," the Arabic for which النتر not resembling any of the variations given. The arrangement and construction of his sentence closely resembles the Bodleian *Almagest*, and is the exact translation (except this one word) of the several copies of Al Sūfi. The verb in these MSS. is يضرب, which corresponds exactly with "tendens" in Gerard of Cremona, and "vergit" in Hyde's Ulugh Beigh. It is not an improbable suggestion that Gerard of Cremona found in his MS. the same untranslatable word which I have found in the MSS. quoted; and one case being the principal star of *Scorpio*, "the accursed constellation and the balęful source of war and discord," and the other, the principal star of *Orion*, who was held to "portend tempests and misfortune," he translated it by the word "rapine," having the *astrological* significance of these constellations and of these particular stars. And, as has been suggested to me by Dr. Garnett, Liechtenstein's addition of "quia appropinquat ad terram in humero Orionis," was a continuation of the astrological idea erroneously started by the mistranslation by Gerard of Cremona of الجوصي, or one of the words given above.

With regard to the British Museum Arabic *Almagest* it appears to differ from the Bodleian *Almagest*, so as to indicate a different translation from the Greek.

I have taken out from this MS. the positions of stars in many of the constellations, and have found a very singular mistake made by the original translator of the MS. from the Greek. The number 60 is expressed in Arabic by the letter س, Seen, but it is curious that the letter ص, Sād, which signifies 90, has always been put for the س. So a translator would record the latitudes of many stars as 90 and odd degrees instead of 60 and odd.

This mistake is rather inexplicable, but it appears that the numerical value of the letters of the alphabet is remembered among the Arabs by certain fictitious words as follows:—

ابجد Abjad,	هوز Huz,
4, 3, 2, 1,	7, 6, 5,
حطي Hati,	كلمن Kalman,
10, 9, 8,	50, 40, 30, 20,
سعفص Sāfas,	قرشت Karshat,
90, 80, 70, 60,	400, 300, 200, 100,
ثخذ Thakhath,	ضظغ Dathagh,
700, 600, 500,	1000, 900, 800,

In the case in question, the scribe, wanting the character for 60, would remember the word "sāfas," which begins and ends with an S sound, and he has taken the last S instead of the first.

Less easy is it to explain a more curious mistake which occurs in both Latin MSS. of the *Almagest* of Gerard of Cremona at the British Museum, and with precisely the same stars as the above error in the Arabic *Almagest*. The latitudes of 34 stars in the Catalogue are given as 300 and odd degrees instead of 60 and odd * degrees. Here the translator must have confounded the س, Seen, =60, with the ش, Sheen, =300, and given the numerical value of the latter letter to the former.

1885, *January* 7.

Note on the Periodic Time of α Centauri.

By A. M. W. Downing, M.A.

In the *Monthly Notices* for November last there is an interesting paper by Mr. Powell on the periodic time of *α Centauri*, in which he contends that the period of this celebrated binary—for our knowledge of the orbital motion of which we are so much indebted to his own observations—cannot, in all probability, be less than 86 or 87 years. Into the reasons which have induced Mr. Powell to come to this conclusion I do not propose to enter on the present occasion, and will content myself with saying that it appears to me that the only test of an orbit is to compare it with *observations*; and that, as far as I can judge, a period of about 76 years will stand this test as well as can be reasonably expected, taking into account the insufficient means of observation employed for the earlier measures, and the difficulty of making accurate observations, on account of the proximity of the stars, in some of the later ones. In order to show this agreement in some extreme cases, I have compared the position-angles computed from the orbit, published in the *Monthly Notices* for March 1884, with the individual measures made before Herschel's time, and also with a set of measures made last year by Mr. Tebbutt (*Observatory*, vol. vii. p. 296) subsequently to the date of publication of the elements to which I refer.

Epoch.	Observer.	Observed Pos. Angle.	Observed—Computed.
1752·2	Lacaille	218°·7	+4°·19
1822·0	Fallows	209·6	−1·00
1824·0	Brisbane	215·4	+3·77
1826·01	Dunlop	213·2	+0·52
1830·01	Johnson	215·0	+0·10
1831·00	Taylor	215·9	+0·41
1832·16	,,	216·4	+0·18
1884·533	Tebbutt	199·80	−0·46

* In the Sloane MS. the impossibility of such latitudes seems to have occurred to the scribe, who has endeavoured to correct them by erasing the cypher, and so making them 30 and odd degrees, but in no case has the correct latitude been given.

I may remark that there is no use in forcing an agreement with Lacaille's position-angle (at all events within 5° or so), as I find that a change of 3″ in the difference of Declination of the components changes the position-angle more than 5°; and considering the small number of observations on which Lacaille's places depend, the quantity mentioned above is probably well within the limits of error. Brisbane's position-angle appears to be erroneous. The agreement in the other cases is close, and I see no evidence that a period differing much from 76·222 years is required to satisfy the observations.

Note on Prof. Pritchard's Comparison of the Light Transmitted by Refracting and Reflecting Telescopes. By W. S. Franks.

There are a few points in Prof. Pritchard's remarks, at the last meeting of the Society, that I should like to see cleared up. At the outset he seems to infer that nothing has been done in the way of careful comparison, between reflectors and refractors, since the time of Dr. Robinson, on whose results he depends largely.

Possibly no photometric comparison had been previously attempted; but, certainly, careful eye-estimates have been made by various observers of the relative light-ratio of the two classes of telescopes, mounted side by side, and directed upon identical objects, the results being published in some of the scientific journals. A cursory inspection of the last twenty volumes of the *English Mechanic* will afford ample evidence in support of this statement.

Dr. Robinson's conditions may be very well so far as they go, but they do not go far enough. Two very important points are ignored—the relative foci of the telescopes, and their magnifying powers. Now I note that Prof. Pritchard stated, in answer to Mr. Rand Capron (*Observatory* report) that the focal lengths of the O.G. and mirrors were the same. This, surely, is a slip, for, according to the best of my knowledge, the particulars are as follows:—

		Aperture.	Focus.
a	Grubb refractor	12½ inches.	15 feet.
b	De la Rue reflector	13 „	10 „
c	With reflector	13 „	9 „ (?)

(The last I am not certain of, but have given the usual focus of Mr. With's 13-in. specula.)

Therefore, the angles of convergence between 15 feet and 9 or 10 feet focus will be widely different. From this it follows that to get equivalent magnifying powers we must use very different eyepieces; and the eyepiece on the refractor would be

formed with lenses of shallower curves, consequently thinner and more transparent, than would be possible with the eyepiece producing an equal amplification on the reflector.

Such comparisons, however carefully made, possess but little value unless the following conditions be attended to—1. The foci must be identical, and the angles of convergence therefore similar; 2, the *same* eyepiece must be alternately used on each instrument, for even eyepieces are not infallible; 3, the instruments must be placed side by side, under precisely similar atmospheric conditions, and directed on the same object; 4, the observer, it is scarcely necessary to add, must be the same. I cannot allow that these conditions were rigidly fulfilled in Prof. Pritchard's comparison. Moreover, I do not consider the principle of extinction by a coloured wedge at all applicable in settling this vexed question. I have tested this on some double stars, and was rather astonished to find such a difference between the relative magnitudes of the components, as seen with the wedge and without it. But if a wedge be used, the stars selected should be *white*. Out of Prof. Pritchard's list only *two* are really white stars, the rest being more or less coloured. [There is one star I fail to recognise, *γ Pegasi*, but, as it is a star of 2·5 mag., perhaps Prof. Pritchard will give its better known Bayer synonym.].

I am aware that Prof. Pritchard states that "he had not entered upon the general question of comparing achromatic object-glasses with mirrors," although, at the same time, he admits that the particular telescopes used "may be regarded as representative instruments." Nevertheless, I do not see how he can escape the logical conclusion that the general is involved in the particular. In all these comparisons I have spoken of the telescopes were regarded as complete instruments, not merely as separate object-glasses or specula. If the latter were the case, we should have also to consider another factor—the secondary mirror or prism used with all reflectors, save the Herschelian. But most practical observers wish to know the ultimate effect of their telescopes as such, caring less about the theoretical difference between O.G. and mirror *per se*. And, evidently, in comparing two telescopes together, it is not fair to use a different eyepiece on each; for how are we to distinguish between effects due to the O.G. or mirror from those produced by the eyepiece alone?

Another statement, on p. 31, *Monthly Notices*, I must take exception to. It is there stated that "it has long been known that the reflective power of silver exceeds that of speculum metal, and this also no doubt is the case (though possibly not to an equal extent) with chemically deposited silver on glass." I now refer to the sentence used parenthetically. The deposit of silver on glass is chemically *pure*, and, with a dense film, takes a perfectly black polish, capable of the highest reflective power; whilst ordinary silver is always alloyed, and, though increasing

in hardness, its reflective properties are thereby somewhat impaired. In connection with this, I may call attention to a statement by Mr. De la Rue, in the discussion which followed the reading of the paper, to the effect that "speculum metal reflects more actinic rays than silver on glass." If this be so, how is it that speculum metal mirrors impart a distinctly *redder* tone to the image than silver on glass?

Much as I would welcome any addition to our knowledge on the interesting, though controversial, subject of the light-ratio between reflectors and refractors, I am afraid we are doomed to wait a little longer before the question shall be finally set at rest.

Leicester:
1884, *Dec.* 13.

Observations of Stars occulted by the Moon during the Eclipse of Oct. 4, 1884, *made at Clapham.* By Edmund J. Spitta.

General Observations.—The sky was cloudless, but owing to the glare of the Moon no stars before No. 74 of the Pulkova list could be recognised with the 10-inch Calver Reflector, power 60. During totality the Moon was, generally speaking, exceedingly faint—indeed, at times barely visible to the naked eye—and presented none of the coppery colour usual on these occasions. It was bluish at the lower edge as seen in the inverting telescope about 10 o'clock, when the other portion seemed brighter than at any time. No markings were plain enough to be recognised.

An assistant called half seconds by the chronometer, time being observed an hour previous, and the observation reduced for collimation and azimuth errors, &c. G.M.T. was obtained by allowing 33 sec. W., taken from the 6-inch Ordnance chart. Owing to the observations being taken without a chronograph, no attempt was made for closer notation of the contacts; indeed, the nature of the phenomena does not seem to permit of greater accuracy—hence the similitude of the decimals in the corrected times.

Star. Pulk. List.	Mag.	G.M.T. Disappearance.	G.M.T. Reappearance.
74	9·3		9 50 4·83
76	10	9 16 52·83	
81	9·5	9 33 33·83	10 38 7·83
82	10	Observed, but badly noted.	10 15 52·83

Star. Pulk. List	Mag.	G.M.T. Disappearance.	G.M.T. Reappearance.
85	10	9 24 40·33	10 31 37·83
94	9–10	10 6 50·83	
95	9·5	10 12 10·33	
106	10	10 36 44·83	
108	9–10	10 37 32·83	

Note of an Observation of Saturn, Nov. 23, 1884.
By Edmund J. Spitta.

Whilst viewing *Saturn*, Nov. 23, with the 10-inch Calver Reflector, my attention was directed, by indirect vision, to an unusual illumination of the edge of the crape ring at the western or following elongation. On applying the *occulting eyepiece* and hiding the ball I found the brightness unmistakable; it lasted some little time, and then the ring resumed its normal aspect. The brightness was ill-defined all the while, but it gave me the impression I was looking at a star through the crape ring, or possibly some satellite whose orbit must lie *within* the rings between them and the ball. The observation was so unexpected that I cannot state its exact duration, but I believe it lasted about 10 minutes, time 11 25 to 11 35 $\pm$ 1 min. G.M.T. Mr. Bryant has computed the exact position of ball and rings, and I have looked over several catalogues, but only find one star near enough to allow of the possibility of occultation. My friend, Mr. Coleman, however, has carefully, and he says under the most favourable conditions, examined the computed position, and finds four small stars, one of them the 11th magnitude, which lies, as nearly as he can judge, in the exact path of *Saturn*. His observation was made near the meridian.

I do not for one moment state I feel positive this brightness was due to a star, but its short duration and the finding of four in or about the path are facts pointing thereto; I merely note the observation, acting up to the motto of the Society.

Clapham Common.

Observations of Comet c 1884 (Wolf), made by the Rev. Ad. Müller, at Stonyhurst College Observatory, with the 8-inch Telescope and Ring-Micrometer.

(*Communicated by the Rev. S. J. Perry, F.R.S.*)

1884.	Stonyhurst Mean Solar Time.	R.A. ♂ − ✶	Decl. ♂ − ✶	No. of Comp.	Apparent R.A.	Log. of Par. Factors.	Apparent Decl.	Log. of Par. Factors.	Star.
	h m s	m s	′ ″		h m s		° ′ ″		
Nov. 10	6 8 23	+1 39·65	− 4 59·9	3	22 30 22·37	8·956$_n$	−0 4 28·4	0·852	1
13	6 24 15	−1 43·19	+18 46·6	5	22 37 21·12	8·782$_n$	−1 1 16·5	0·857	2
14	6 25 34	+0 38·62	+ 2 27·5	5	22 39 42·90	8·750$_n$	−1 18 25·7	0·859	3
19	9 48 3	−0 42·75	+23 10·7	3	22 51 37·69	9·380	−2 37 22·5	0·862	4
20	6 55 35	+1 57·62	+10 38·9	5	22 54 18·09	7·498	−2 49 54·4	0·867	5
21	6 26 56	+1 0·00	+18 23·9	4	22 56 59·85	8·587$_n$	−2 59 38·7	0·868	6
22	9 32 21	−3 38·85	−13 15·9	3	22 59 29·88	9·356	−3 17 41·0	0·865	7
23	6 4 5	−4 3·89	+14 53·8	3	23 1 11·51	8·830$_n$	−3 28 25·2	0·870	8
24	6 15 31	−1 33·68	+ 2 32·8	3	23 3 41·62	8·683$_n$	−3 40 46·3	0·871	9
24	7 15 18	−1 27·72	+ 2 5·8	3	23 3 47·58	8·631	−3 41 13·3	0·871	10

Dark Field illuminating Micrometer, with two cross wires at Right Angles to each other, and inclined 45° to the Parallel of Declination.

	h m s	m s	′ ″		h m s		° ′ ″		
Dec. 9	7 3 49	+0 29·97	+ 6 32·0	5	23 42 28·48	8·751	−5 45 57·0	0·880	11
11	9 53 11	−3 18·50	+ 9 41·2	4	23 47 56·06	9·453	−5 55 51·7	0·869	12
14	9 58 47	−1 39·55	− 3 56·1	3	23 55 44·74	9·457	−6 7 4·1	0·868	13
15	8 37 16	+0 45·03	− 3 36·7	4	23 58 9·32	9·300	−6 9 44·3	0·857	14

Assumed Mean Places for 1884·0 *of the Comparison Stars with Reductions to Apparent Place for the Night of Observation.*

Star.	R.A. 1884·0 + Reduct.	Decl. 1884·0 + Reduct.	Authority.	Remarks.
	h m s s	° ′ ″ ″		
1	22 28 39·56 + 3·16	+ 0 0 5·8 + 25·7	Lalande 44096	Comet well defined.
2	22 39 1·16 + 3·15	− 1 20 28·2 + 25·1	„ 44495	Good sky. Comet fainter.
3	22 39 1·16 + 3·12	− 1 20 28·2 + 25·0	„ 44495	Fine and clear.
4	22 52 17·31 + 3·13	− 3 0 57·2 + 24·0	Weisse (hora 22) 1052	Star of comparison rather doubtful.
5	22 52 17·31 + 3·16	− 3 0 57·2 + 23·9	„ 1052	Bright clear sky. Nucleus fainter.
6	22 55 56·71 + 3·14	− 3 18 26·2 + 23·6	Lalande 45036	Good.
7	23 3 5·56 + 3·17	− 3 4 48·7 + 23·6	„ 45297	Satisfactory.
8	23 5 12·23 + 3·17	− 3 43 42·1 + 23·1	„ 45360	Definition good.
9	23 5 12·23 + 3·16	− 3 43 42·1 + 23·0	„ 45360	Observation difficult ; Moon and mist.
10	23 5 12·23 + 3·16	− 3 43 42·1 + 23·0	„ 45360	Observation more satisfactory.
11	23 41 55·38 + 3·13	− 5 52 49·2 + 20·2	Weisse (hora 23) 821	Nucleus very poorly defined.
12	23 51 11·39 + 3·17	− 6 5 52·8 + 19·9	Lalande 46940	Wind high. Clouds passing. Observation very difficult.
13	23 57 21·13 + 3·16	− 6 6 27·2 + 19·2	Weisse (hora 23) 1152	Nucleus invisible. Measures made with difficulty.
14	23 57 21·13 + 3·16	− 6 6 27·2 + 19·1	„ 1152	Observation good.

The observations of this comet were commenced on October 12, and continued up to the present time on every available occasion. Owing to the faintness of the comet it was impossible to use the bright-field micrometer. Hence the observations of October 12, 14, 15, and 19 were made with the square-bar micrometer by taking transits, the declinations being read at the circle of the Equatorial. As, however, such positions are not sufficiently exact, they have not been inserted in the preceding list.

During the whole period of the observations the comet has been a sufficiently distinguishable object, consisting of a round luminous patch, having a diameter of from 1′ to 1′·7, with a nucleus situated somewhat towards the N.W. of the centre. This bright patch sometimes presented the appearance of having luminous projections, emanating from the side opposite to the nucleus. This was especially the case on November 10 and 14 when they assumed the appearance of a V.

The first series of measures were made with a ring-micrometer, sufficiently well constructed at the Observatory. The second series, dating from December 9, were made with a bright-wire micrometer, the wires being at 45° to the diurnal motion. The wires were placed very exactly in position by means of the position-circle of the telescope, which is read by two verniers.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1884.

(*Communicated by the Astronomer Royal.*)

Day of Obs.	Phenomenon.	Telesc.	Power.	Moon's Limb.	Mean Solar Time of Observation.	Obs.
Feb. 6 (*a*)	Disapp. 120 Tauri	E. Eq.	140	Dark	8 59 6·30	H.
Mar. 6	Disapp. λ Geminorum	E. Eq.	70	„	10 9 31·89	H.
6	Disapp. λ Geminorum	Altaz.	100	„	10 9 31·99	A. D.
May 8 (*b*)	Disapp. λ Virginis	E. Eq.	140	„	9 13 15·42	H.
30 (*c*)	Disapp. 16 Sextantis	Simms' Eq.	220	„	10 17 47·21	A. D.
Dec. 30	Disapp. 115 Tauri	E. Eq.	140	„	8 28 20·56	H. T.

(*a*) Disappearance instantaneous. (*b*) Some cloud about the Moon.
(*c*) Disappearance instantaneous.

Phenomena of Jupiter's Satellites.

Day of Obs.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observation.	Mean Solar Time of N.A.	Obs.
					h m s	h m s	
1884, Jan. 11	II.	Tr. Ing. Bisection	E. Eq.	70	8 22 3	8 25 0	H.
		Last contact	,,	,,	8 25 48		
24	I.	Tr. Ing. First contact	,,	140	6 48 51	6 49 0	L.
		Last contact	,,	,,	6 53 1		
	I.	Tr. Egr. First contact	,,	,,	9 5 9	9 9 0	L.
		Last contact	,,	,,	9 10 48		
Feb. 12 (*a*)	II.	Tr. Ing. First contact	,,	70	6 27 35	6 31 0	H.
		Last contact	,,	,,	6 34 29		
	II.	Tr. Egr. First contact	,,	,,	9 25 46	9 25 0	H.
		Last contact	,,	,,	9 30 36		
15	I.	Occ. D. Last seen	,,	,,	9 9 53	9 9 0	A. P.
	I.	Ecl. R. First seen	,,	,,	12 5 44	12 5 28	A. P.
21 (*b*)	II.	Ecl. R. First seen	,,	140	8 16 17	8 16 9	L.
		Full brightness	,,	,,	8 16 32		
26	II.	Tr. Ing. First contact	,,	,,	11 9 37	11 10 0	L.
		Last contact	,,	,,	11 12 51		
29	I.	Occ. D. Bisection	,,	70	12 41 37	12 43 0	H.
		Last seen	,,	,,	12 44 9		

Day of Obs.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N.A. h m s	Obs.
1884, Mar. 4 (*c*)	II.	Tr. Ing. First contact	E. Eq.	210	13 30 25	13 32 0	H.
		Last contact	,,	,,	13 34 0		
(*d*)	IV.	Ecl. R. First seen	,,	70	14 12 15	14 15 42	H.
		Full brightness	,,	,,	14 15 33		
6 (*e*)	II.	Ecl. R. First seen	,,	210	13 27 18	13 27 27	A. D.
9 (*f*)	III.	Occ. R. Bisection	S.E. Eq.	310	7 36 25		W. C.
		Last contact	,,	,,	7 37 40	7 41 0	,,
(*g*)		Clear of planet	E. Eq.	70	7 42 23		T.
(*h*)	III.	Ecl. D. First contact	S.E. Eq.	310	8 18 10		W. C.
		Bisection	,,	,,	8 20 40		,,
		Last seen	,,	,,	8 26 25	8 23 44	,,
		Began to fade	Simms' Eq.	220	8 17 38		L.
		= Sat. I. in brightness	,,	,,	8 21 42		,,
		Last seen	,,	,,	8 24 57		,,
10	I.	Tr. Egr. First contact	E. Eq.	70	8 35 59	8 38 0	H.
		Last contact	,,	,,	8 40 18		
12 (*i*)	IV.	Tr. Ing. First contact	Simms' Eq.	220	7 43 5		W. C.
		Bisection	,,	,,	7 47 50		,,
		Last contact	,,	,,	7 53 50	7 41 0	,,
(*k*)		First contact	E. Eq.	210	7 41 18		A. D.
		Last contact	,,	,,	7 52 46		,,

Day of Obs.	Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N.A. h m s	Obs.
1884, Mar. 12 (*l*)	IV.	Tr. Egr. First contact	E. Eq.	210	11 57 6	12 8 0	A. D.
		Last contact	,,	,,	12 12 3		
15	II.	Tr. Egr. Bisection	Simms' Eq.	220	8 1 7	8 5 0	T.
		Last contact	,,	,,	8 3 21		
21 (*m*)	IV.	Ecl. R. First seen	S.E. Eq.	310	8 14 59	8 22 19	M.
(*n*)		First seen	Simms' Eq.	220	8 19 28		W. C.
22	II.	Tr. Egr. Last contact	E. Eq.	210	10 34 21	10 34 0	A. D.
24	II.	Ecl. R. First seen	Simms' Eq.	220	7 56 2	7 56 6	T.
		Full brightness	,,	,,	7 57 32		
	I.	Tr. Ing. First contact	,,	,,	10 1 27	9 59 0	T.
		Last contact	,,	,,	10 5 56		
(*o*)	I.	Tr. Egr. Last contact	,,	,,	12 20 34	12 19 0	T.
29	II.	Tr. Ing. Last contact	E. Eq.	70	10 5 37	10 11 0	H.
31	II.	Ecl. R. First seen	,,	140	10 32 1	10 31 26	L.
		Full brightness	,,	,,	10 33 58		,,
		First seen	Simms' Eq.	220	10 32 19		H.
		Full brightness	,,	,,	10 34 23		,,
	I.	Tr. Ing. First contact	,,	,,	11 48 10	11 52 0	,,
		Bisection	,,	,,	11 50 40		,,
		Last contact	,,	,,	11 54 59		,,
		First contact	E. Eq.	140	11 51 15		L.
		Last contact	,,	,,	11 55 25		,,

Day of Obs.		Sat.	Phenomenon.	Telesc.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N.A. h m s	Obs.
1884, Apr.	1 (*p*)	I.	Ecl. R. First seen	E. Eq.	210	12 34 21	12 34 13	T.
	2	I.	Tr. Egr. First contact	,,	70	8 37 12	8 39 0	H.
			Last contact	,,	,,	8 43 11		
	3 (*q*)	III.	Tr. Egr. Bisection	Simms' Eq.	220	8 48 55		A. D.
			Last contact	,,	,,	8 52 40	8 50 0	,,
			Last contact	E. Eq.	210	8 52 53		T.
	7 (*r*)	II.	Occ. D. First contact	Simms' Eq.	220	7 41 6	7 43 0	A. D.
			Last contact	,,	,,	7 45 6		
	9 (*s*)	I.	Tr. Ing. First contact	E. Eq.	210	8 14 18	8 13 0	T.
			Last contact	,,	,,	8 20 27		
		I.	Tr. Egr. Last contact	,,	,,	10 34 5	10 33 0	T.
	10 (*t*)	I.	Ecl. R. First seen	Simms' Eq.	220	8 58 26	8 58 36	M.
			First seen	E. Eq.	70	8 58 39		H.
		III.	Tr. Ing. First contact	,,	,,	9 5 4		,,
			Last contact	,,	,,	9 13 53		,,
			First contact	Simms' Eq.	220	9 9 56	9 12 0	M.
			Bisection	,,	,,	9 14 36		,,
			Last contact	,,	,,	9 18 16		,,
	16 (*q*)	I.	Tr. Ing. Last contact	,,	,,	10 12 50	10 8 0	H.

Day of Obs.	Sat.	Phenomenon.		Telesc.	Power.	Mean Solar Time of Observation h m s	Mean Solar Time of N.A. h m s	Obs.
1884, Apr. 21 (*b*)	III.	Ecl. D.	Began to fade	Simms' Eq.	220	8 19 15	8 23 10	M.
			Half brightness	,,	,,	8 21 45		,,
			Last seen	,,	,,	8 25 10		,,
			Last seen	E. Eq.	140	8 22 59		S. D.
	III.	Ecl. R.	First seen	,,	,,	11 48 26	11 53 55	,,
(*n*)			First seen	S.E. Eq.	310	11 54 31		M.
			Half brightness	,,	,,	11 56 46		,,
			Full brightness	,,	,,	11 59 26		,,
23 (*v*)	IV.	Occ. R.	First seen	,,	220	8 14 21	8 15 0	,,
			Bisection	,,	,,	8 18 24		,,
(*r*)			Last contact	E. Eq.	210	8 19 11		A. D.
	I.	Tr. Ing.	First contact		,,	12 2 19	12 3 0	
			Bisection	,,	,,	12 4 34		,,
			Last contact	,,	,,	12 7 39		
24 (*w*)	I.	Occ. D.	First contact	Simms' Eq.	220	9 12 14	9 15 0	L.
			First contact	E. Eq.	140	9 9 48		H.
28	III.	Occ. R.	First seen	,,	,,	10 45 43	10 49 0	L.
			Last contact	,,	,,	10 47 58		,,
			First seen	Simms' Eq.	220	10 45 50		H.
			Last contact	,,	,,	10 50 25		,,
30	II.	Tr. Ing.	Last contact	E. Eq.	140	9 59 11	10 0 0	,,
May 3	I.	Ecl. R.	First seen	,,	210	9 14 15	9 13 53	A. D.
			Full brightness	,,	,,	9 17 2		

Day of Obs.	Sat.	Phenomenon.		Telesc.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time of N.A. h m s	Obs.
1884, May 9	I.	Tr. Ing.	First contact	E. Eq.	210	10 25 35		
			Bisection	,,	,,	10 28 44	10 26 0	B.
			Last contact	,,	,,	10 32 19		
10	IV.	Ecl. D.	Began to fade	,,	140	10 1 33		L.
			Bisection	,,	,,	10 3 3		,,
(*b*)			Last seen	,,	,,	10 5 7	10 2 25	,,
			Last seen	Simms' Eq.	220	10 5 13		W. C.
(*x*)			Last seen	S.E. Eq.	,,	10 6 50		M.
10 (*y*)	I.	Ecl. R.	First seen	,,	,,	11 9 25		,,
			Bisection	,,	,,	11 11 41		,,
			Full brightness	,,	,,	11 13 53	11 9 21	,,
			First seen	E. Eq.	140	11 9 47		L.
			Full brightness	,,	,,	11 11 57		,,
23 (*c*)	III.	Tr. Ing.	Last contact	,,	,,	9 56 45	9 55 0	A. P.
26	I.	Ecl. R.	First seen	Simms' Eq.	220	9 29 15		A. D.
			Full brightness	,,	,,	9 32 9	9 29 2	,,
			First seen	E. Eq.	140	9 29 41		B.
Nov. 24 (*z*)	I.	Ecl. D.	Began to fade	,,	70	14 10 18	14 10 48	H.
			Last seen	,,	,,	14 10 57		
Dec. 6	III.	Ecl. D.	Last seen	Simms' Eq.	220	15 47 52	15 43 55	H. T.
(*b*)			Last seen	E. Eq,	140	15 47 3		L.

Notes.

(*a*) Slightly cloudy.
(*b*) Observation considered very good.
(*c*) *Jupiter's* limb indistinct.
(*d*) Very distinctly seen.
(*e*) Satellite appeared at full brightness four minutes after recorded time.
(*f*) Satellite not seen till 15ˢ before time recorded for bisection; sky hazy; definition poor.
(*g*) Satellite just clear of planet; cloudy.
(*h*) Satellite had become extremely faint at $8^h\ 26^m\ 10^s$, but was still visible by glimpses for 15ˢ longer. Definition good; edge of *Jupiter's* shadow seen sharp and distinct on the satellite. Time for first contact probably too late. Time of bisection fairly exact.
(*i*) The last contact had certainly not occurred at $7^h\ 53^m\ 30^s$, and was long past at $7^h\ 54^m\ 50^s$; clouds passing.
(*k*) Clouded over immediately after time noted for last contact, but observer considered that the last contact had then taken place.
(*l*) Planet very unsteady; satellite quite clear of *Jupiter* 2^m after time of last contact.
(*m*) *Jupiter* frequently lost in cloud, but at the moment when the satellite was first seen the planet was shining very brilliantly. The satellite was so faint at the first glimpse, and was so long before it showed a sensible disk, that there can be no doubt that the first instant of reappearance was well caught. At $8^h\ 20^m\ 49^s$ the satellite was at half brightness, at $8^h\ 24^m\ 19^s \pm$ at $\frac{3}{4}$ brightness. Clouds intervened before these times, and the planet was lost in cloud before the satellite had gained its full brightness. When glimpsed for a few seconds about $8^h\ 25^m\ 19^s$ the satellite was not even then so bright as Satellite II.
(*n*) Extremely faint when first glimpsed; steadily and very distinctly visible 30ˢ later. Sky clouded over about $8^h\ 24^m$.
(*o*) *Jupiter* diffused; sky very thick and hazy.
(*p*) Definition bad; sky thick.
(*q*) Cloudy at first contact.
(*r*) Cloudy; observation uncertain.
(*s*) Definition very bad; sky foggy.
(*t*) Satellite at half brightness $1^m\ 15^s$ after recorded time, and at full brightness $2^m\ 20^s$ after recorded time.
(*u*) *Jupiter* low down and in some amount of mist and smoke; image very unsteady.
(*v*) Considered a good observation. *Jupiter* in mist and cloud constantly passing, but just for a moment or two the planet was well seen with the minutest point of the satellite projecting from the limb. Observation of bisection very rough; *Jupiter* faint. Clouds concealed the planet before the satellite was clear of the limb.
(*w*) Observation not good; satellite very faint; cloudy. *Jupiter* clear of cloud at $9^h\ 20^s$, when there was no sign of the satellite.
(*x*) Considered a very good observation. Satellite steadily held though exceedingly faint to moment of disappearance. Sky very clear; images fairly steady.
(*y*) Considered a very good observation. Sky very clear, but images very unsteady.
(*z*) Satellite seemed to reappear for an instant about 10ˢ later.

The clear aperture of the object-glass of the S.E. Equatorial is 12·8 inches; of the E. Equatorial, $6\frac{3}{4}$ inches; of the Altazimuth, 4 inches; and of the Simms' Equatorial, 6 inches.

The initials W. C., H. T., A. D., M., T., L., H., A. P., B., and S. D., are those of Mr. Christie, Mr. Turner, Mr. Downing, Mr. Maunder, Mr. Thackeray, Mr. Lewis, Mr. Hollis, Mr. Pead, Mr. Bennett, and Mr. Dolman.

Occultations of Stars by the Moon, and Phenomena of the Satellites of Jupiter and Saturn, observed at Mr. Edward Crossley's Observatory, Bermerside, Halifax, in the year 1884, *with the* $9\frac{1}{3}$*-inch Cooke Refractor.* By Joseph Gledhill, F.R.A.S.

Lunar Occultations.

1884.			G.M.T. h m s		
Oct.	4	No. 61	9 22 42	R.	Power 62: bad sky.
		No. 63	9 35 55	R.	Mr. Crossley was at the telescope and Mr. Gledhill at the chronometer.
		No. 76	9 28 2	R.	
		No. 85	9 25 52	D.	
		No. 82	9 25 40	D.	
			10 37 28	D.	Small double star; angle about 140° from N.
			10 45 36	D.	Small star; angle about 45°.
			9 44 30	D.	„ „ 170°.
	5	*o* Piscium	9 8 0		Near approach; distance from nearest point of limb 3′ 14″. Power 60.
	12	*α* Cancri	18 1 33	D.	Power 240.
Nov.	5	111 Tauri	9 9 5	D.	Power 62.
		115 Tauri	10 2 7	D.	„
			10 46 40	R.	„
	7	68 Gemin.	11 15 59	D.	„
			12 16 19	R.	„
	9	*h* Leonis	13 59 59	D.	Thin cloud near Moon.
			15 4 35	R.	Clear sky. Power 62.
	11	76 Leonis	The star escaped occultation.		
	25	*θ* Aquarii	5 39 7	D.	Bad sky. Power 62.
			6 52 12	R.	„ „
	29	*o* Piscium	The star escaped occultation.		
Dec.	3	B.A.C. 1930	Watched from 10.10 till 10.51; no star seen near Moon.		

					G.M.T. h m s	N.A: h m s	
1884, Jan.	7	I.	Ec. D.	Just gone	11 21 25	11 21 14	Fair definition.
	11	II.	Sh. I.			8 0 0	Shadow very faint; not well seen till 8.15 P.M.
		II.	Tr. I.	Outer contact	8 22 ±	8 25 0	Good definition now and then.
				Bisection	8 24 ±		
				Inner contact	8 26 ±		
		II.	Sh. E.	First contact	10 50 ±	10 54 0	Definition poor now.
		II.	Tr. E.	,,	11 13 ±	11 19 0	Better definition.
				Bisection	11 15 ±		
				Second contact	11 18 ±		
	15	I.	Tr. I.	Inner contact	10 41 ±	10 39 0	Much cloud.
	19	III.	Oc. R.	Just off?	8 2 ±	8 1 0	Badly seen through cloud.
	20	II.	Oc. R.	,,	8 36 ±	8 35 0	No trace of Sat. at $8^h\ 33^m$.
	25	I.	Ec. R.	First seen	6 21 40	6 21 35	Seen through thin cloud.
	26	III.	Oc. D.	First contact	7 40 ±	7 41 0	Stormy.
				Bisection	7 42 ±		
				Just gone	7 45 ±		
Feb.	2	III.	Oc. D.	First contact	10 58 ±	10 58 0	Poor definition.
				Bisection	11 1 ±		
	2	III.		Just gone	11 3 ±		
	6	III.	Sh. E.	Certainly off	6 2 ±	6 0 0	Cloudy.
		I.	Oc. D.	Half gone	13 0 ±	12 58 0	,,
	8	I.	Ec. R.	First seen	10 10 44	10 10 42	Seen through thin cloud.

				G.M.T. h m s	N.A. h m s	
1884, Feb. 9	I.	Tr. E.	Inner contact	7 2 ±	7 3 0	Stormy.
	I.	Sh. E.	Off	7 33 ±	7 34 0	,,
21	II.	Ec. R.	First seen	8 16 8	8 16 9	Observed at the Observatory of James S. Cooke Esq., Gomersal, near Leeds. 7-in. refractor.
			Fully bright	8 19 ±		
24	I.	Ec. R.	First seen	8 29 4	8 29 10	Much motion.
			Full disc	8 33 ±		
Mar. 1	I.	Tr. I.	First contact	10 2 26	10 2 0	Much cloud. Mr. Crossley at telescope.
			Bisection	10 4 6		,, ,,
			Inner contact	10 5 51		,, ,,
	I.	Sh. I.	Just within	11 0 ±	10 58 0	,,
8	II.	Sh. E.	Inner contact	7 37 ±	7 42 0	,,
			Half off	7 39 ±		
9	III.	Ec. D.	Fading	8 21 0	8 23 44	Cloud obscured planet.
10	I.	Tr. I.	First contact	6 19 30	6 18 0	
			Inner contact	6 24 0		Uncertain.
15	II.	Tr. E.	Inner contact	7 59 30	8 5 0	Good sky.
			Bisection	8 1 0		
			Outer contact	8 3 0		
	II.	Sh. I.	Bisection	7 25 0	7 24 0	E. C. at telescope.
			Just within	7 26 0		,,
	II.	Sh. E.	Inner contact	10 14 0	10 19 0	
16	III.	Oc. D.	First contact	7 44 30	7 47 0	

				G.M.T. h m s	N.A. h m s	
1884. Mar. 16	I.	Oc. D.	First contact	11 49 0	10 49 0	
			Half gone	11 50 30		
			Just gone	11 52 0		
	III.	Oc. R.	First seen	11 15 0	11 20 0	
			Half off	11 16 0		
			Just off	11 17 30		Uncertain.
	III.	Ec. D.	Fading?	12 18 0		
			Fading	12 20 0		
			Half bright?	12 22 30		
			Just gone	12 27 1	12 24 7	
18	I.	Ec. R.	First seen	8 43 6	8 43 27	Clouds pass.
			Half bright	8 45 0		
			Full disc	8 47 0		
21	IV.	Ec. R.	First seen	8 16 34		
			Still faint	8 20 0	8 22 19	
			Not half disc	8 22 0		
			Full disc?	8 30 0		
24	II.	Ec. R.	First seen	7 50 6	7 56 6	Clouds.
	I.	Tr. I.	First contact	9 59 0	9 59 0	
			Second contact	10 4 0		
25	I.	Ec. R.	First seen	10 39 1	10 38 49	E.C. Perhaps a few seconds late.
	I.	Oc. D.	First contact	7 6 30	7 8 0	
			Gone	7 10 0		

				G.M.T. h m s	N.A. h m s	
1884, Mar. 31	II.	Ec. R.	First seen	10 31 38	10 31 26	Probably 10ˢ late.
			Half disc	10 34 0		Uncertain.
			Full	10 37 0		,,
Apr. 2	I.	Tr. E.	Just off	8 40 0	8 39 0	Clouds.
	I.	Sh. E.	Inner contact	10 0 0	9 55 0	,,
	I.	Sh. I.	,,	7 37 0	7 35 0	Uncertain.
7	II.	Oc. D.	Outer contact	7 42 0	7 43 0	
			Half gone	7 44 0		
			Just gone	7 45 30		
Apr. 7	II.	Ec. R.	First seen	13 6 41	13 6 40	
9	I.	Tr. I.	Outer contact	8 16 0	8 13 0	Bad definition.
			Bisection	8 18 0		Uncertain.
			Inner contact	8 19 30		
	I.	Sh. I.	,,	9 31 0	9 30	
	I.	Tr. E.	,,	10 29 0	10 33 0	
			Bisection	10 32 30		
			Outer contact	10 34 30		
	I.	Sh. E.	Inner contact	11 45 0	11 50 0	Uncertain.
May 10	IV.	Ec. D.	Fading	9 58 0		
			Just gone	10 4 10	10 2 25	
	I.	Ec. R.	First seen	11 9 50	11 9 11	
			Full disc?	11 12 0		
18	I.	Tr. E.	Half off	9 12 0	9 14 0	

				G.M.T. h m s	N.A. h m s	
1884, May 18	I.	Tr. E.	Just off	9 15 0		
26	I.	Ec. R.	First seen	9 29 5	9 29 2	
			Half full?	9 31 0		
			Full	9 32 30		
Nov. 8	I.	Ec. D.	Fading fast	15 56 23	15 56 35	Clouds.
9	I.	Tr. I.	Outer contact	14 15 30	14 17 0	
			Inner contact	14 18 ±		Planet very low.
11	III.	Sh. I.	Just within	13 43 ±	13 41 0	Good sky.
	III.	Sh. E.	,,	17 23 ±	17 24 0	
13	II.	Ec. D.	Last seen	18 15 34	18 15 26	Bad sky.
15	II.	Sh. I.	Just within	13 9 ±	13 10 0	
	II.	Tr. I.	First contact	15 39 ±	15 41 0	Bad sky.
	II.	Sh. E.	Just within	16 6 0	16 7 0	
	I.	Ec. D.	Fading	17 48 0	17 49 36	
			Just gone	17 50 6		
16	I.	Sh. I.	,,	14 54 0	14 57 0	Uncertain: cloud.
	I.	Tr. I.	First contact	16 10 0	16 12 0	
			Last contact	16 13 0		
17	I.	Oc. R.	First seen	15 49 0	15 50 0	No definition.
22	II.	Sh. I.	Just on disc	15 44 0	15 43 0	Uncertain.
	III.	Oc. R.	First seen	16 27 0	16 28 0	Much cloud.
24	I.	Ec. D.	Fading	14 8 0	14 10 48	Cloudy.
29	III.	Ec. R.	First seen	15 18 57	15 19 19	Misty.

Observations of the Elongations and Conjunctions of the Satellites of Saturn.

Aperture $9\frac{1}{3}$ inches; power 240.

October 6, 1884.—Much cloud often; bad definition.
Enceladus, S.; $13^h\ 10^m$, not on line; $13^h\ 40^m$, near; $13^h\ 50^m$, on line? $14^h\ 0^m$, past.
Rhea, N.; $13^h\ 0^m$, just on line? clouded.

October 7 1884.—*Tethys*, E.; $15^h\ 0^m$, not up; $15^h\ 5^m$, up? $15^h\ 7^m$, up? $15^h\ 10^m$, past.
Dione, S.; $19^h\ 30^m$, not up; $19^h\ 40^m$, not up? $19^h\ 50^m$, on line; $20^h\ 0^m$, past.

October 14, 1884.—*Rhea*, elongation E.; good definition; $10^h\ 45^m$, G.M.T., not up; $10^h\ 50^m$, nearly up; $10^h\ 55^m$, on the web; $11^h\ 0^m$, past? $11^h\ 5^m$, certainly past.

October 19, 1884.—*Dione*, E.; definition bad; $11^h\ 30^m$, not up; $11^h\ 35^m$, nearly on line? probably on it; $11^h\ 40^m$, certainly past.

October 20, 1884.—*Enceladus*, E.; bad definition; $15^h\ 50^m$, near the web; $15^h\ 54^m$, probably on the web; $15^h\ 58^m$, past? $15^h\ 59^m$, certainly past.

October 28, 1884.—Elongation E. of *Tethys*; $9^h\ 15^m$, not up; $9^h\ 20^m$, near; $9^h\ 25^m$, on line? $9^h\ 30^m$, past? $9^h\ 35$, past.

November 5, 1884.—*Tethys*, N.; bad sky; wind and cloud; $9^h\ 50^m \pm$.

November 7, 1884.—*Enceladus*, conjunction S.; bad sky; clouds; $11^h\ 0^m$, not up; $11^h\ 5^m$, on line? $11^h\ 10^m$, past?

November 11, 1884.—Good definition.
Enceladus, elongation E.; $13^h\ 45^m$, not up; $13^h\ 50^m$, up? $13^h\ 50^m$, on line; $14^h\ 0^m$, past? $14^h\ 5^m$, past.
Mimas, W.; glimpsed now and then; on line, $13^h\ 50^m \pm$.
Rhea, conjunction N.; $15^h\ 40^m$, not up; $15^h\ 45^m$, on line; $15^h\ 50^m$, certainly past.

November 19, 1884.—Good definition; *Tethys* easy; *Rhea* rather difficult.
Tethys, conjunction S.; 13^h, not up, but near; $13^h\ 15^m$, up? $13^h\ 25^m$, up; $13^h\ 30^m$ past.
Rhea, elongation E.; $13^h\ 30^m$, $13^h\ 40^m$, not up; $13^h\ 50^m$, up? $14^h\ 0^m$, up? $14^h\ 10^m$, past.

November 20, 1884.—Bad definition, but plenty of light.
Tethys, N.; $11^h\ 45^m$, $11^h\ 55^m$, not up; $12^h\ 0^m$, up? $12^h\ 5^m$, up? $12^h\ 10^m$ past; $12^h\ 15^m$, past.
Dione, S.; $14^h\ 5^m$, $14^h\ 10^m$, not up; $14^h\ 15^m$, up? $14^h\ 20^m$, up? $14^h\ 25^m$ past.
Rhea, N.; $16^h\ 10^m$, $16^h\ 15^m$, not up; $16^h\ 17^m$, up? $16^h\ 20^m$, up? $16^h\ 24^m$ on line; $16^h\ 30^m$, past. *Enceladus* not seen.

November 22, 1884.—Much cloud at times.
Tethys, N.; $9^h\ 10^m$, not up; $9^h\ 15^m$, near; $9^h\ 17^m$, very near; $9^h\ 19^m$ to $9^h\ 25^m$, up? $9^h\ 30^m$, past? $9^h\ 33^m$, past.
Enceladus, E.; $12^h\ 30^m$, just visible; not up; $12^h\ 40^m$. very near; $12^h\ 42^m$, very near; $12^h\ 45^m$, on line? $12^h\ 50^m$, on line? $13^h\ 0^m$, past? $13^h\ 5^m$ past; well seen at $13^h\ 30^m$. *Mimas* invisible.

November 23, 1884.—$8^h\ 10^m$, two satellites seen at times; one a little past S. conjunction at $8^h\ 10^m$.

November 24, 1844.—Much cloud; *Dione*, N., $16^h\ 47^m \pm$.

November 25, 1884.—*Enceladus*, N.; on web, $13^h\ 49^m$; past $13^h\ 51^m$.

November 26, 1884.—*Mimas* not seen.
Enceladus, E.; very difficult; $15^h\ 34^m$, not up; $15^h\ 40^m$, past.
Dione, E.; $18^h\ 45^m$, not up; $18^h\ 50^m$, on web? clouds passing.

November 27, 1884.—*Dione*, N.; $10^h\ 15^m$, not up; $10^h\ 20^m$, not up; $10^h\ 35^m$, on line? $10^h\ 40^m$, on line? $10^h\ 45^m$, past.
Rhea, S.; $10^h\ 35^m$, $10^h\ 45^m$, not up; $10^h\ 48^m$, up; $10^h\ 49^m$, up; $10^h\ 50^m$ to $11^h\ 0^m$, up; $11^h\ 5^m$, past.

November 28, 1884.—*Enceladus*, N.; $7^h\ 25^m$, not quite up; $7^h\ 30^m$, up? $7^h\ 35^m$, past?

November 29, 1884.—*Rhea*, E.; $11^h\ 0^m$, $11^h\ 10^m$, not up; $11^h\ 20^m$, $11^h\ 25^m$, on line? $11^h\ 30^m$, past? $11^h\ 35^m$, past.
Dione, E.; $12^h\ 15^m$, $12^h\ 20^m$, not up; $12^h\ 25^m$, up? $12^h\ 30^m$, past? $12^h\ 35^m$, past.
Rhea, N.; $16^h\ 50^m$, not up; $16^h\ 55^m$, up; $17^h\ 0^m$, up? $17^h\ 10^m$, past.

December 6, 1884.—*Rhea*, S.; $11^h\ 15^m$, not up; $11^h\ 20^m$, not up; $11^h\ 25^m$, up? yes; $11^h\ 28^m$, past.

December 9, 1884.—*Enceladus*, N.; $6^h\ 20^m$, not up; $6^h\ 40^m$, past; very difficult.
Tethys, N; $8^h\ 45^m$, near; $8^h\ 50^m$, up; $8^h\ 50^m$, past? $8^h\ 55^m$, past.

December 15, 1884.—*Enceladus*, S; on line, $10^h\ 15^m$?
Rhea, N.; $11^h\ 50^m$, not up ? $11^h\ 55^m$, not up ? $12^h\ 0^m$, up ?
$12^h\ 10^m$, up ? $12^h\ 15^m$, past ? $12^h\ 20^m$, past.

Method of Observation.—Usually the horizontal section of the eye was placed parallel to the longer diameter of the rings, and a judgment formed as to when the satellite was at its elongation E. or W., or on an imaginary line passing through the centre of the ball from N. to S. Occasionally the web of the micrometer was used to aid the estimation, and when definition was very good, the straight edge of a diaphragm between the lenses of the eyepiece was sometimes used.